THE AMERICAN
JOURNAL OF PSYCHOLOGY
Founded in 1887 by G. STANLEY HALL

Vol. XLVII JULY, 1935 No. 3

THE PATIENT'S PSYCHOLOGICAL SITUATION UPON ADMISSION TO A MENTAL HOSPITAL

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A well established movement in psychiatry, represented by such names as Adolf Meyer, C. Macfie Campbell, and Harry S. Sullivan, stresses the importance of environmental factors in the genesis and in the further course of a psychosis and tries to reconstruct the picture of these determining influences from the reports of various informants. It might be even more promising to study these factors through direct observation of the patient. As the only place where the behavior of the mentally ill can be observed directly and systematically is the mental hospital, it is only here that the patient can be studied in relation to his actual environment. No serious attempts known to the writers have been made to investigate systematically *the hospital situation as it exists for the patient,* that is to say, *the psychological situation of the mental hospital.* Such a task would necessitate the abandonment of the view that the behavior of the patient can be only understood if his psychosis is constantly taken as a basis. Rather one must try to understand the patient as a person brought into a new and peculiar environment—the mental hospital—and has to find his own place in it.

The present study is an attempt to accomplish one part of this task by studying the psychological situation of newly admitted patients. The study has resulted in the finding of six types of hospital-situations, each of which accounted for many aspects of the behavior of a group of patients. These groups will be described in the first part of the article. The system of psychological concepts on which the grouping is based and the discus-

* Accepted for publication June 15, 1934. This study was completed by the aid of a Grant from the Social Science Research Council.

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sion of the groups in terms of these concepts will be found in the second part.

To study the actual entrance of the patient into the hospital, systematic observations were made on new patients during admission and during the succeeding two to seven days. Various routine procedures were observed. The one which provided the bulk of material for the present study was the reading of a general letter of information, signed by the superintendent, which is given to each patient within the first twenty-four hours after his admission. This letter describes the hospital and the patient's future position in it, and thus presents an incentive for him to express his attitude towards the institution. Behavior during the reading of the letter and comments made on various points provide valuable clues for reconstruction of the actual psychological situation of the patient in so far as it is determined by his hospitalization.[1]

The observations of all procedures, including the reading of the letter, were carried out on the admission-ward on 100 consecutively admitted new patients equally divided as to sex. (Distribution as to diagnosis is given in the appendix.) In order to preserve the customary ward-situation, the authors tried to play some subordinate rôle in connection with every procedure they observed, and took notes inconspicuously on everything which was done or said. In the situation involving the reading of the letter, they acted as messengers, and, when necessary, as interpreters of the letter. Even though they answered all questions and in general entered into a conversation whenever the occasion demanded, they never asked any questions and thus kept the situation clear from any resemblance to an examination. The general principle followed during observation was to have the situation determined as far as possible by the behavior of the patient and to act according to the particular requirements of each single case.

Description of the Psychological Groups

Group A: Primitive drive out of the hospital. (a) The blind attack. There are patients who, finding themselves locked up in the hospital, try to get out of it immediately, attacking everything which may possibly be an obstacle. In hunting for a means of escape, they run towards every door which is opened, try to force the locks; implore the nurse, the doctor, and every new person they see to open the doors and to let them out. They request that the superintendent be called immediately in order to set them free. They insist that they must go home right away, refusing to wait until tomorrow. All these attempts at liberation are wholly ineffective. The patients do not stop to consider the expediency of their behavior.

[1] The effects of the letter itself are discussed in detail in a separate study entitled The value of an orientation letter for newly admitted patients, *Psychiat. Quart.*, 8, 1934, 703-721.

They display strong affect; shouting, yelling, banging the door; all sorts of protesting actions are observed.

A very pronounced form of this behavior was observed in women who were anxious to go home to their children, and in the case of a drug addict who demanded immediate satisfaction of his craving for drugs. The primitive manifestations of this strong urge never last very long. Only in exceptional cases are they continued for an indefinite length of time. We observed one woman whose psychosis manifested itself in innumerable repetitions of the question, "Can I go home today?" and in attempts to run out of every door which was opened. Her whole activity was reduced to little more than these manifestations.

(b) Despair. The 'despair' type of behavior is closely related to the type just described. The patient becomes discouraged by the repeated failure of his attempts to get out, or even from the beginning of his hospitalization thinks that the obstacles are insurmountable. He loses hope of ever achieving his purpose, and stops struggling altogether in order to avoid further painful failures. He cannot completely withdraw, however, from contact with the objects and people that stand as obstacles to his going home. Everything in his environment, even mere mention of topics connected with his hospitalization, reminds him of the limitations of his situation and of his own weakness. The obstacles which he is thus obliged to face without being able to attack them, acquire for him the character of relentless enemies. They may even seem to approach and threaten to crush him. The resulting behavior varies from mild sadness, apprehension, and suspiciousness through complete despair and affective breakdown, *e.g.* bitter crying. The patient interprets everything as a threat that he will be forever buried in the institution. Even the implication of insanity is for him not an offense as for many others, but rather an insurmountable obstacle to his going home. As such it is desperately protested against: "I am not crying because I am crazy, but because I am sad."

The fact that the behaviors described under (*a*) and under (*b*) often show a quick alternation, seems to indicate that they are closely connected with each other. The first kind of behavior is of an active fighting type which implies at least a minimal hope of winning, the second is a despairing defeat; but both are characterized by the affective manifestations, the lack of objective insight, and a certain degree of hostility and fear.

(c) The roundabout-way. The affective attempts to get out of the hospital in the quickest way, if they have taken place at all, are soon discontinued. The patient gives up direct opposition for an attitude of compliance with the requests of those in charge. Even if he does not fully understand their purpose, he is ready to do whatever they require, hoping by his obedience to secure their patronage and eventually obtain from them permission to

go home. This permission seems to the patient to depend completely on the power of the individual nurses or doctors, and he eagerly looks for clues as to which behavior will meet with the approval of these authorities. They stress their willingness to be "good patients:" "I will work, I will tell them everything they want to know, I want to go home!" This behavior is merely a means to an end and is compatible with the maintenance of an inimical attitude towards all representatives of the hospital. More often, however, giving up the open fight makes the person more accessible also to the positive sides of hospital life. Members of personnel lose their indiscriminate character as obstacles and acquire a specific character which corresponds to their immediate action with regard to the patient. Those who act in a friendly way are promoted, in his eyes, to the rank of well-meaning people, and he conceives the possibility of achieving his purpose with their help. The endeavors of the hospital personnel are, therefore, of the utmost importance for the development of this attitude, which may be summed up as follows: the patient no longer tries to get out immediately, but starts on a way which in comparison with his first attempts seems to be roundabout, the shortest way having proved a blind alley. He shows willingness to obey and to become temporarily hospitalized, considering it the only possible way out of the hospital, and giving up, partly at least, the attitude of suspiciousness and fear. This behavior may be either the result of a development or it may be displayed from the beginning of the hospitalization.

Although the attitude just described leads to more intelligent behavior than the blind attack and the despair, a certain intellectual primitivity is common to all patients of this group. They are mostly led by concrete aspects of the situation and do not appreciate its more abstract and more general implications, such as their legal status or general hospital rules. The causes of their hospitalization are for them immediate concrete events or persons: "I should not have got into this car, that was the beginning of all the trouble;" "My brother was mad with me. He told the doctors to keep me here." Accordingly, their roundabout way is also of a concrete and primitive character. They choose a definite behavior only because it is approved by those in authority, without a clear insight into the more remote reasons of this approval. The hospital situation is constituted for them by the immediate surroundings, by the admission ward. The goal for which they are striving is also sharply defined; the patient wants to go home in order "to put the children to bed," "to give them their Sunday clothes," "to collect the potato crop." He or she protests not against hospitalization as such, but only against being kept away from these places and these tasks. This was strikingly demonstrated by the request of one woman who asked for the permission to go home for two hours to feed the chickens, and promised to come back right away. In their social relations the patients of this group are strongly dependent on a direct and personal relationship and often fail to distinguish the official action of an employee from a personal one.

Group B: Insight into the basic factors of hospitalization. There are patients who, on being admitted to the hospital, show a behavior very different from the quite primitive conduct of the group described above. Though their greatest desire is to leave the institution, these patients do not display any affect; they are calm and reserved, coöperative and friendly in an impersonal way. They make no excessive claims to the time and help of the personnel and are not very communicative in personal matters. Their attitude is that of inquiry, not of protest and struggle. They try to find out why they were brought to the hospital, how long they will have to stay, what their legal status is, and other things relative to leaving the institution. When this information is given them they seem relieved and express satisfaction.

Both the behavior and the remarks of a member of this group show that, while keenly aware of the limitation of his momentary position, he goes beyond the mere fact of these limitations to the more general factors constituting the situation. He shows a more or less adequate understanding of the reasons for his hospitalization. He realizes that he is only one of the inmates of the institution and as such subject to the general rules of the place. Moreover, he wants and is able to understand the necessity and the purpose of these rules. He knows that the attendant will not let him out, because his release depends on other factors than personal good-will and possession of keys, and he has some insight into these factors. He realizes the existence of a hospital hierarchy, superintendent, doctor, nurse, and attendant, and is aware of their respective degrees of authority. He also knows that the procedure which may result in his liberation takes time and that he has to await his turn. In other words, for a patient of this group the hospital situation is not determined by the immediate surroundings: he sees it in the first place as a social organization which imposes certain restrictions. According to this prevalence of the general aspects of the situation, the patient's aim is also defined quite generally as the regaining of personal freedom. As he sees it, he is kept from the immediate realization of this aim by rules and laws pertaining to hospitalization and by respective authorities of the officials representing these laws.

The wider outlook of these patients permits them to see ahead, and thus the future becomes very real for them. The future, as these patients see it, holds out much better prospects for them than their present situation. They are convinced that they are going to be discharged soon, and this idea is not a mere wish or hope, but a quite definite belief. Analyzing their reasons for believing in the termination of their hospital stay, we find that these reasons, though they may prove invalid in a given case, are in themselves sound and give evidence of good insight into the structure of the situation. The patients may realize that the hospital is a place of cure, and trust that they will be dismissed because they are well or as soon as they are well. They may believe that the reasons for which they were brought to the hospital (such as alcoholism, drug addiction, attempts at suicide) are not sufficient to keep them there. Some of them were brought for observation with the promise of their relatives that they would be taken home

soon; others are readmissions who, on the basis of their previous experience, expect a release within a short time, and some are court cases who know that they have been sent to the hospital for a short observation-period.

These findings explain the apparently paradoxical fact that these patients, while wishing to be at home as strongly as the members of the first group, yet do not become emotionally disturbed. This may be understood on the assumption that, owing to the wider outlook of these patients, their psychological situation includes the future and that, for one reason or another, the future situation includes the end of the hospital stay.

Group C: Refusal to accept the reality of the hospital situation. The attitude of a small, but quite outstanding group of patients is characterized by their refusal to acknowledge the full import of their actual situation in the hospital. This refusal may vary from a simple protest that one does not belong here to the almost complete disregard of the fact that one actually *is* here.

In the first case, the person, finding himself locked up in a mental hospital, displays the behavior of one being unjustly treated. He is either indignant and keeps aloof from the surroundings or puts up a fight by argument, trying to prove by elaborate reasoning his complete mental health and the illegality of his hospitalization. In the second case, the person more or less disregards the fact that he is actually being looked upon and treated as a mental patient. His whole behavior is meant to show that his mental health is far beyond any possible doubt, both for himself and for others. He, therefore, refrains from all explicit arguments about his condition, merely implying by his behavior that he belongs to the normal group. The patients of this type do not admit even the possibility of permanent incarceration and have some explanation ready at hand, which gives their stay in the hospital the appearance of a harmless episode, a casual misunderstanding, a need of a period of rest. They make the best of this involuntary 'trip to the hospital' and use the opportunity for collecting impressions and making observations on 'that part of humanity which the other part does not know anything about.' They are more curious and condescending, however, than sympathetic towards the other patients. Towards the employees they assume a superior attitude. While patients who explicitly protest against their hospitalization are contemptuous, critical, fault-finding, and sometimes sarcastic, these last described are benevolent and condescending. They graciously consent to submit to the usual routine, make appreciative remarks about the hospital, treat the employees with cordial politeness and even promise them their patronage in the future. This attitude, when it has at its disposal the resources of the acute psychotic state, may go very far; one patient assumed the rôle of the superintendent of a new hospital to be founded, revelled in detailed projects about it, hired employees and, in the mean time, anticipating her future position, made suggestions for the reform of this hospital.

Another form of this behavior and at the same time the last step in the negation of the hospital situation, is exemplified by the case of the patient who does not even condescend to the least attempt to impress anyone by demonstration of her normality and of her superiority. Though perfectly oriented in the momentary situa-

tion, she completely withdraws from the surroundings, intimating only that she expects to go out any moment, but eventually reveals that the forces on which she relies are supernatural and that she is the one elected by God. Yet most of these patients in unguarded moments suddenly seem to be personally touched and show depressed behavior very different from their usual talkative and cheerful mood. One cannot help thinking that they in some way realize the importance of their stay in the hospital and only assume the rôle of superiority, trying to persuade others, and in that way themselves as well, that they are entirely unconcerned about the state of things.

In order to understand the behavior of the patients of Group C, we have to consider one aspect of hospitalization which as yet has not been sufficiently stressed. The hospitalization not only robs the patient of his liberty for a shorter or longer period of time, but it carries with it also the implication of insanity, with all its consequences. A person with a high degree of self-respect, coming to a mental hospital suddenly realizes or even fearfully anticipates in advance that he will be treated as a mental patient, *i.e.* an irresponsible being who has no say even in his own affairs, whose manifestations are never taken seriously, and on whom everybody looks down and thinks himself right in doing so. This unbearable position of social inferiority, of not being taken as full and equal to anybody, can become for the patient the predominant problem of his hospitalization. This problem touches the person so deeply that in comparison with it the question of mere physical stay in the hospital has only a secondary importance.

The prevalence of the 'social inferiority' of hospitalization typical for the members of this group is consistent with their general high level of aspiration. A closer investigation of their life histories shows that their claims to high intelligence, literary and artistic achievement, professional efficiency, heroic conduct, position in society, and political influence are frequently not quite groundless though sometimes they are of a purely imaginative nature. Moreover, their chief claim, the claim of normality, is sometimes confirmed by the final diagnosis.

Group D: Preoccupation with psychosis. Some patients at the time when they are brought to a hospital are absorbed in their psychosis, attempting to see through their new and overwhelmingly strange experiences. Every conversation that one starts with them leads up to the problem of their psychotic experiences, which alone seem to interest and to move them deeply. Though the patient is perfectly oriented and behaves according to the requirements of the situation, the psychotic content may break in at any moment. He suddenly interrupts a conversation, trying to demonstrate the electric sparks coming from his fingers or kneels for a moment, obeying the voices which he hears. In talking about these events he seems thrilled and overawed at the same time. Very often what he says reveals that, while deeply involved in and moved by the psychotic events, he still faces them as something strange and foreign to him. When the observer, on hearing about the patient's newly acquired electric power, remarks: "These are strange experiences!" he readily agrees: "Yes, I would never have

believed it myself before that such things were possible!" The psychosis is still a problem for the person; it remains his chief preoccupation even in the new situation of the hospital.

The actual confinement, the loss of independence, and the depreciatory opinion of others are of minor importance in comparison with this central problem. This does not mean that the patients are quite indifferent towards their real situation. On the contrary, their state may make them more open to their surroundings and at the same time more vulnerable. They talk openly about personal matters and look anxiously for sympathetic understanding. They are very sensitive and notice every shade of tone and meaning, positive as well as negative. For the most part, they readily comply with what others request of them, but on the other hand they are very easily hurt. They may complain bitterly of the rude treatment, the inattention and indifference of the personnel, the lack of comfort and rest for their exhausted minds and bodies. Sensitive as they are to the unpleasant aspects of their situation, their general attitude towards the hospital is, however, determined not by these aspects, but by their *attitude towards their psychosis*. According to this attitude two subtypes may be distinguished.

(a) Partial insight. With some patients this attitude is characterized by attempts to ward off the disease. They have some insight into the abnormality of their new experiences, or at least a feeling of the impending disease. "I had a nervous breakdown, did not feel all right." "I wish my mind went straight!" At the same time the patient is deeply involved in the psychotic experiences which are important and valid for him, and does not separate them from himself. Taking his stand in these experiences he sees his situation as a state of rejection by God, a state resulting from his previous sinfulness—the psychosis appears to him as his own "terrible mistake." Yet, sharing the opinion of the outside observers, he recognizes the same facts as constituting his sickness, for which he tries to give a natural explanation, such as great strain, over-work, being upset by the family.

As a result of this partial insight, the patients, though they may resent having been brought to the hospital against their will, realize the actual reason for their hospital stay. They hope to find help and cure, and express willingness and even anxiety to stay: "I like to be in a place like this." "I hope they will make me well. I don't care what they do to me. . . . I hope everything will be all right." Their double standpoint towards the psychosis finds its expression also in their conception of getting well; and getting well is equivalent to the solution of their problems which are given by the content of the psychosis, *e.g.* becoming reconciled with God, or proving one's innocence. Therefore, the

demands for cure alternate with the similarly pressing demands for help in solving these problems: "Will they help me? It is so hard to find anybody who can settle all this terrible mix-up. I am not a bad man. I did not want all this disturbance. It is hard to disentangle it!" It might be that the predominance of this second conception of the mental recovery accounts for the fact that the patients do not seem actually to believe in the possibility of help from the outside, though they are eagerly looking for proofs to the contrary, saying for example, "I suppose if the cases are very bad they can do nothing, they cannot cure them?"

(b) No insight. Other patients of this group have gone one step farther in making the psychosis their own. Though for them, too, the psychotic experiences are still something strange and unusual, they do not have the negative character of an illness, of something of which one had to rid one's self. On the contrary, these new powers of ideas fascinate and absorb the patient. He responds to every hint at the abnormal origin of these experiences with affective outbursts: "Well, that gets me! I am not worrying over anything, I am not sick. I would like them to find another person who is as healthy as I am at the present moment!" The implication of insanity is protested against, not as a social inferiority but as an invalidation of the important new experiences.

The patient is rather indifferent to everything outside of his psychosis, and does not actually object to the stay in the hospital—as long as he is well treated. The hospital for him is a more or less indifferent background for his psychotic experiences, a place where he can pursue in peace his religious and philosophic meditations or his poetic activities. Single factors of the hospital situation may acquire a more specific connection with the psychotic events. The patient welcomes the psychiatric interviews which give him the opportunity to communicate to others his new discoveries. The work that he is asked to do in the hospital is looked upon as helping him to reach perfection, and even a stay on a ward for disturbed patients may temporarily take on a positive meaning, that of a trial which helps to moral improvement. At the same time all decidedly positive and decidedly negative aspects of the momentary situation (such as physical comfort, friendly treatment on the one side, and discomfort, disturbance by inmates, humiliating experiences on the other) are keenly sensed and responded to in an adequate way. If the unpleasant events accumulate, the negative side becomes prevalent and the patient may begin to want to leave the hospital, to which in general he is quite indifferent.

Group E: Hospital as a place of refuge. Among the newly-admitted patients there are persons in whose past history factors were at work which gradually made life in the community more and more difficult and painful for them. This is true in the case of patients who, because of their asocial behavior, have been repeatedly sent to corrective or psychiatric institutions or have been placed under state control, of patients who have

been out of work for a long time, of those who because of old age have lost their families, have nobody to take care of them and do not feel competent to take care of themselves. For these people there exists no situation which they would decidedly prefer to that of the hospital. If they were discharged they very often would not know where to go or would immediately get into trouble. They are aware of this fact and therefore the hospital acquires for them the character of a refuge. At the same time the hospitalization does not completely lose its negative character. This conflicting situation accounts for all that is contradictory, unclear, and unbalanced in the behavior of the single patient. It accounts for individual differences as well. The attitude of this group towards hospitalization fluctuates between seeing it as inevitable necessity and seeing it as their own free choice. On the other hand, it fluctuates between negative and positive evaluation of the hospital. The attitude of the patients can be best described by considering them according to these characteristics.

On the one end of the scale are the patients who have been brought to the hospital much against their will but who very soon realized that this was the only possible arrangement. They become resigned and attempt to adjust on this new and lower level of life. This attitude is typical for those of the senile group who realize that there is no other place for them. Even though seen as inevitable, the stay in the 'insane asylum' retains for them a negative character. "I don't want to die here," said one old woman, and another wished the observers to have a happy life and never to become patients of a State Hospital.

We next find patients who are not so sure that their hospitalization was an inevitable necessity, the only way out of the situation. They feel that to a certain degree they were free to choose this or another situation, and in fact there are voluntary and half-voluntary commitments in this group. In this case the conflict reaches its climax. The patient, having chosen the hospitalization as the best solution of his problem (this may have been unemployment, prison, compulsive anti-social behavior), is anxious to be kept in the hospital. He seeks better contact with his surroundings and expresses his appreciation of the institution and his willingness to work and to do all that is required of him. On the other hand, he very keenly realizes the limitations of this situation and is highly sensitive to them, especially to his position of inferiority: "What I hate most in this place is that they do not take one seriously. At times nurses talk to me as if I had no brains at all . . . that's what gets me!" The patient tells over and over again how hard it was for him to make up his mind to come to the hospital. He stresses, thus trying to persuade himself as well as others, the fact that it was absolutely necessary and that he could not help it. Actually, however, the patient never loses sight of the other alternative. At times another decision seems to have been possible to him and he half regrets that he did not adopt it. "Maybe I ought not to have consented to come. Now my record is quite spoiled. I have been in the Psychopathic Hospital, in prison, and in the State Hospital!"

At the extreme of this group is one patient who, at the time of admission,

showed no regret whatever and most emphatically manifested his desire to stay in the hospital. This man, after having spent three years in the hospital and finally having succeeded in being discharged, felt that he was persecuted and violently abused by his own family. On being readmitted he was still in a marked affective state. During his account of what he had suffered at the hands of his family, he started crying and declared that he wanted to stay in the hospital forever, that he would like to work here. Although actually he was sent back by the family, he looked upon hospitalization as a result of his own free choice and did not seem to have any misgivings about the step. It is evident that this appreciation of the institution was induced by contrast with the very unsatisfactory conditions that existed outside, and thus the consent to hospitalizaion was, even in this case, a forced one.

For Group E in general, it was striking that even in the cases when the patient made uncontrollable conditions (unemployment, illness, old age) responsibile for his stay in the hospital, he showed a strong sensitivity about these points and spoke of them in an apologetic tone as if ashamed of his own infirmities: "I kind of got old and had much trouble, my things got stolen." This note of personal involvement becomes even stronger in patients who blame the hate of the family or the ingratitude of society for their misfortune. Others seem to think that the cause of their unhappiness is closely connected with themselves, with their inefficiency, bad temper, or lack of energy. To find a way out of this situation would mean to become another person.

Group F: The narrowed situation. In this group the common characteristic is that the hospital situation is narrowed to its immediate concrete aspect and therefore its essential limitations, which are of a more general nature, disappear. The patient may protest against this or that part of the routine, but he does not protest against hospitalization as such. Some minor agreeable or disagreeable happenings determine his behavior from moment to moment.

The behavior of one patient of this group, for instance, was dominated on admission by her fear of possible operations and of medical procedures in general. She was easily quieted and comforted, however, when more pleasant sides of institutional life were pointed out to her. Accordingly, after having read the informational letter about the hospital, she was found in a fit of apprehensive anger, shouting that she would not stay in this "damn place," would not have her teeth pulled out (a remark referring to the paragraph about dental service). But when the messenger mentioned other points of the letter she quickly quieted down and enumerated correctly a whole list of topics pertaining to life in the institution, such as physical examination, size of institution, return of own clothes, cafeteria, store, dances, and movies. As she proceeded to the more pleasant points she became more and more cheerful and at the end seemed quite thrilled with all the pleasures that awaited her. None of the really important points of the letter were touched

upon. With patients of this type who react in a quite lively fashion to their immediate surroudings, this complete lack of forethought and insight into the wider implications of the momentary situation gives the impression of a striking inadequacy. In fact, the diagnosis at least considers mental deficiency in all of these cases.

This group includes also patients who, at the time when the observers came to see them, were in a state of excitement. Though they willingly agreed to interrupt their incessant speeches and to listen to the reading of the informational letter, they were too much preoccupied with their own ideas to get more than a few isolated points of the information given to them. The general meaning of this information was not grasped, the communication was considered as an irrelevant and more or less annoying interruption of a much more important occupation. "My goodness! I listen to a whole book and do not understand half of that stuff!" It might be assumed that the real hospital situation as a differentiated whole practically did not exist for these patients, or at least it was not accessible to observation.

The concrete momentary aspects of the situation also prevail over the more general ones in the patients who feel physically ill and have no desire to leave their beds. They either do not realize the character of the institution, or if they do, they do not care about it. Their situation is practically not different from that of any patient of a general hospital who is in very poor condition. The questions of nursing care are the only ones for which some interest is shown.

Finally, should be mentioned the group of patients (Group F_a) whose psychological situation could not be ascertained because of lack of response. This group is composed—with a few exceptions— of senile, of mute, and of physically sick patients in extremely poor condition. It may be assumed that if it were possible to determine their psychological situation, it would in most cases be nearer to that described in this section than to that of any other group of patients.

Theoretical Discussion

The system of psychological concepts which served as a theoretical basis for the present study is that of the topological and dynamical psychology of Lewin.[2] For purposes of clarity, the theoretical discussion of the groups is preceded by definitions of the concepts used.

The concrete psychological situation in which a person finds himself is represented topologically as a group of regions and subregions. The region which represents, for example, the situation of a freshman in college may include, as subregions, attendance at a mathematics class, the football game, a party at a friend's and the like. The regions differ qualitatively, and the line of demarcation between them is called a *boundary*. If the person leaves one region for another we speak of

[2] K. Lewin, Environmental forces, *A Handbook of Child Psychology*, 1933, 590-625; Conflict between Aristotelian and Galileian modes of thought in psychology, *J. Gen. Psychol.*, 5, 1931, 141-177; Vectors, cognitive processes, and Mr. Tolman's criticism, *J. Gen. Psychol.*, 8, 1933, 318-345; and *A Dynamic Theory of Personality*, 1935.

locomotion. The locomotion from one region or field to another may be easy or difficult. In those cases in which the boundary offers resistance to this transition we speak of the boundary as a *barrier.* The resistance of the barrier may vary from zero to infinity. If the freshman wants to visit a friend out of town and easily obtains permission from the college authorities to do so, we may say that the resistance of the barrier is zero or nearly zero; if he obtains it only with much effort, we speak of the existence of a strong barrier; and finally, if all his efforts are in vain, we consider the resistance of the barrier as practically infinite. A barrier may surround one definite field which does not include the person, and thus leave other fields free for him. Any clearly defined prohibition, such as a prohibition against going to the movies, may be used as an example. In other cases the barrier may surround the person and thus limit him to one definite field, excluding all the others. Thus, a student whose work has been found unsatisfactory may be cut off from a wide range of activities and be restricted to the field of scholastic work. In these cases we speak of a surrounding barrier.

Sometimes the psychological situation is more dependent on the person's wishes than on the existing reality, which may be neglected, as in the case of day-dreaming. We consider the situation as lying in the *plane of unreality,* and represent it, utilizing the third dimension, by a region lying over the plane of reality. One of the dynamic characteristics of the plane of unreality is that the resistance of the barriers is reduced.[3]

In addition we shall use the concepts of force, valence, and tension. In using these concepts for the description of the situation we speak of a region as a field. If a person tries to get into a definite field or in general tries to reach some goal, we say that a *force* is acting upon this person in the direction of this field or of this goal. The field or goal is then said to have a *positive valence.* For example; if student *A* on entering the dining-room sees his friend *B* sitting at one of the tables, this place acquires a positive valence for *A.* If there are no additional factors in the situation, *A,* upon whom a force acts in the direction of the positive valence, will go and sit at this table. If, however, instead of his friend he discovers *C* whom he strongly dislikes, this place may have for him a *negative valence;* he will withdraw. If a person is at the same time under the influence of two forces working in opposite directions, we say that he is in conflict. In our example, this would be the case if *A* on entering the dining room should discover both his friend *B* and his enemy *C* sitting at the same table. He would be attracted towards this table and at the same time would wish to withdraw. If the conflict is not solved soon in one way or another, and the person cannot leave the field, a strong *tension* may result which will show itself in restlessness and affectivity.[4]

Fields, boundaries, barriers, locomotions, forces, and tensions are not physical but psychological data. Identical physical fields may serve as a basis for quite different psychological fields, *e.g.* the situation of watching an oncoming rain is not psychologically identical for a tourist and for a farmer. Psychological boundaries need not coincide with the physical boundaries, *e.g.* in order to cross a boundary (*i.e.* to change the situation) one does not have to go out of the room—a new topic of conversation

[3] I. F. Brown, Über die dynamischen Eigenschaften der Realitäts- und Irrealitätsschichten, *Psychol. Forsch.,* 18, 1933, 1-26; T. Dembo, Der Aerger als dynamisches Problem, *ibid.,* 15, 1931, 36-390.

[4] Cf. Dembo, *op. cit.,* 15, 1931, 18-26.

may change the situation completely. Walls and locked doors may assume the rôle of psychological barriers. The same is true of non-material obstacles, such as rules, prohibitions, and social and moral considerations. The kind of objects and relations which constitute the field—its qualitative character, as it were—has to be ascertained in every given situation.

How can the different types of psychological situation of newly admitted patients be explained in terms of topological and dynamical concepts? The following specific questions have to be answered.

What constitutes the psychological field of the patient? Are there any barriers in this field, especially any barriers which surround the person? If there are barriers, what is their qualitative nature and, in particular, how are they related to the barriers set by the hospital? What is the direction of the forces in the field acting on the patient? What tensions and locomotions result from them?

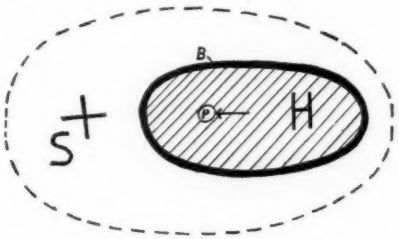

FIG. 1. GENERAL SCHEME OF THE SITUATION COMMON TO GROUPS A-E
The force is represented by an arrow. Tension is represented by hatchings. (In all figures, the greater density of the hatchings means the stronger tension).

P = Person
H = Enclosed field
B = Barrier
S = Outside field
+ = Positive valence of the field

A survey of the groups shows as their most general common characteristic the division of the psychological field into two distinct parts separated by a barrier; an enclosed field in which the person finds himself and an outside field which he would like to enter (Fig. 1). This structure is common to all groups except Group F. The contents of the field—the specific objects and relationships significant for the patient—are, however, different for different groups. Table I gives a comparative survey of these qualitative characteristics. Besides giving the general character of the field, it also shows how this characteristic applies to the enclosed field, the surrounding barrier, and the outside field which the patient wishes to enter. For the rest the chart is self-explanatory.

TABLE I

SURVEY OF THE CHARACTERISTICS OF THE DIFFERENT GROUPS

	Group	Gen'l character of situation and outstanding objects in field	Enclosed field	Surrounding barrier	Outside field
(A)	Primitive drive out of hospital	Concrete individual persons, their friendly or inimical relations, world of concrete objects	Admission ward, nurses, ward-life	Arbitrary will of individuals (doctors, nurses), closed doors, walls	Spatially and temporally defined concrete situation (home with children, feeding chickens tonight, immediate satisfaction of craving for drugs)
(B)	Insight into basic factors of hospitalization	More abstract and general relationships between persons and objects, wider context	Hospital as a social institution defined by its purpose	Rules, laws: Own state as seen objectively	Regaining of independence. Undisturbed performance of work, management of own affairs
(C)	Refusal to accept the reality of hospital situation	Social relationship of dominance and submission, problems of social rank	Position of social inferiority as a mental patient	Depreciatory social opinion of persons of immediate surroundings and of wider social groups	Restitution of their rights. Respect and acknowledgement of their high claims
(D)	Preoccupation with psychosis	World of ideas; metaphysical, ethical, religious problems	Being mentally sick. Being in a state of rejection	The disease. Own guilt, sinfulness	Mental recovery. Moral improvement
(E)	Hospital as a place of refuge	Everything seen in relation to self, as factors of own misfortune	The whole life situation	Bars of circumstance. Own life history	Unreal field of wish fulfillment
(F)	Narrowed situation	Same as in Group A	(No constant enclosed field)	(No constant barriers)	(No constant outside field)

The differences between the psychological situations of the groups imply different degrees of personal involvement. Here this problem can be only touched upon: it deserves a special investigation. The groups, with the exception of the first and the last (A and F), are arranged according to the increasing personal involvement of the patient in the situation. This may best be demonstrated by consideration of the barriers typical for each group. While Group F (narrowed situation) has no constant barriers at all, and for a patient of Group A (primitive drive out of the hospital) the barriers are purely external, in no way connected with himself, in Groups B through E the barriers show an increasing involvement of the person. In Group B (insight into the basic factors of hospitalization) the personal involvement of the patient is but slight. His own state, in so far as it constitutes the barrier (such as organic or even mental disease, or alcoholism), is seen by him objectively, as only one concrete instance of a rather general and widespread condition. In his personal life this aspect is only peripheral and has no permanent significance. In his personal serious conflict about it so long as it does not interfere with the usual course of life of the patient and does not deprive him of his independence. In Group C (refusal to accept the reality of the hospital situation) the personal involvement is stronger. The evaluating critical opinion of the others touches the person more deeply than the application to himself of objective and generally acknowledged rules. It determines to a certain degree his self-esteem and can easily become a source of prolonged conflict. In Group D (preoccupation with psychosis) the barriers are connected with a sphere which in the case of these patients acquires an utmost and exclusive importance for their personal existence; it is the question of protecting one's own personality from being destroyed by the psychosis, which may at the same time be experienced as a struggle for an ideal. In the last case, the problem is that of the ultimate values of personal existence, in comparison with which the opinion of others, one's own reputation, the external limitations appear unimportant. Finally, in Group E (hospital as a place of refuge) the involvement of the person in the barriers is complete, not limited to any one of the more or less peripheral spheres; it is the character of the individual as such which is involved in the structure of the barriers. The realization of one's own personality-structure as the barrier found apt expression in the statement of one patient: "I wish I could take a rest from myself!"

Considering the relation of the psychological barriers to the barriers specific to the hospital, we find that the groups are presented in the decreasing order of importance of the specific hospital barriers for the patient. Roughly speaking, a line of demarcation can be drawn between Group C and Group D (cf. Table I), dividing all observed cases into those in which the psychological barriers and the specific hospital barriers coincide to a certain degree, and into those in which they fall apart. For Groups A, B, and C the psychological barriers are constituted by various factors inherent in the hospital situation, such as locked doors and the commands of those in charge for Group A; general rules and laws concerning hospitalization for Group B; the blame and the social ostracism commonly attached to hospitalization for Group C. For Groups D and E,

on the other hand, the factors constituting the psychological barriers—disease, own failures—are by no means a result of the hospitalization as such. One patient of Group E (a case of voluntary commitment) expressed it very dramatically. Pointing to the iron bars on the windows, he said, "It is not these bars that keep me here, it is the bars of circumstance." Accordingly, the chief desire of the patients of the first three groups is to leave the hospital, while the patients of Groups D and E may be indifferent or even favorable to the stay in the hospital.

After this general survey of the qualitative aspect of the groups we shall consider their more specific dynamical characteristics, that is the forces and barriers present in the field and the specific behavior resulting from them in each group and subgroup.

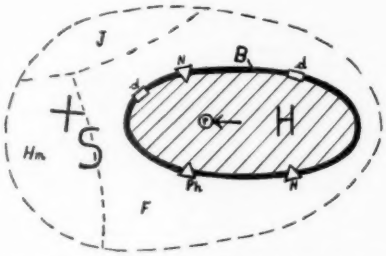

FIG. 2. ILLUSTRATING SITUATION OF 'BLIND ATTACK' TYPE OF BEHAVIOR OF GROUP A

P = Patient
H = The inside of the hospital
B = The walls of the hospital
Ph = Physician
N = Nurse
d = Door
S = Outside of the hospital
Hm = Region "home"
J = Region "job"
F = Region "friends"

The *blind attack behavior* (p. 382) of the subgroup of Group A (primitive drive out of the hospital) presupposes a strong force in the direction out of the hospital and towards a concrete immediate goal, in a situation surrounded by a barrier (Fig. 2). This strong urge prompts the patient to attack the barriers—doors, locks, nurses—which prevent him from leaving the hospital directly and immediately. The stronger the force, however, with which he attacks the barrier, the greater is its resisting force. In this situation a tension results which manifests itself in affectivity. The affectivity prevents the person from perceiving the finer structure of the situation and of the barrier.[5] This is one of the factors which account for

[5] Cf. Dembo, *op. cit., Psychol. Forsch.*, 15, 1931, 113-115.

the patient's failure to use more elaborate and more adequate means for his purpose.

The attitude of *despair* met with in the same group (p. 383) is but another phase of the same dynamic process characterized by strong forces

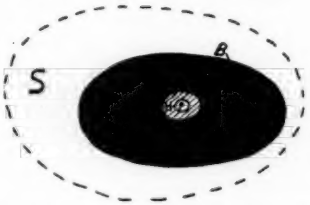

FIG. 3. ILLUSTRATING SITUATION OF 'DESPAIR' TYPE OF BEHAVIOR OF GROUP A
P = Patient
H = The inside of the hospital
B = The extended barrier
S = Outside of the hospital

and tension. This phase is marked by the patient's forced passivity and increased emotionality in the face of insurmountable obstacles which even seem to become active and to threaten him. The psychological situation in this case implies a spreading of the surrounding barriers over the field (Fig. 3). Finally, because of this extension of the barriers, any locomo-

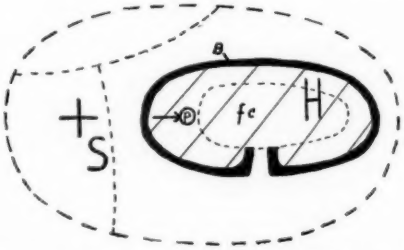

FIG. 4. ILLUSTRATING SITUATION OF 'ROUNDABOUT' TYPE OF BEHAVIOR OF GROUP A
P = Patient
H = The inside of the hospital
fc = Region "full coöperation with the hospital personnel"
B = The walls of the hospital and other barriers (cf. Fig. 2)
S = Outside of the hospital

tion in the direction of the goal becomes impossible or nearly impossible.

In the case of patients who show the *roundabout-way* behavior (p. 383), hoping by obedience to secure permission to go home, the drive to get out of the hospital may be as strong as that underlying the affective behavior.

But in the measure in which the manifest affectivity subsides, the field regains its finer structure, and a certain region segregates itself from others within the enclosed field—the region of full compliance with the requirements (Fig. 4). The patient has to go through this region before he can leave the hospital, even though this presupposes locomotion in the direction opposite to the direction of his previous efforts. The discovery of this roundabout-way out of the hospital transforms the situation from one surrounded by an insurmountable barrier into a relatively open one, and thus results in a further decrease of affectivity.

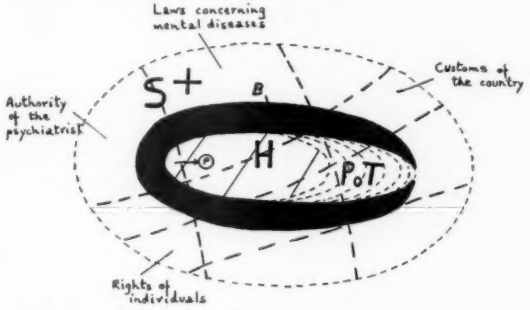

FIG. 5. ILLUSTRATING SITUATION OF PATIENT OF GROUP B

P = Patient
H = Hospitalization
B = Barrier constituted by the laws concerning mental disease, authority of the psychiatrist
S = Personal freedom
PoT = Passage of time

The broken lines that extend over both the enclosed and the outside field indicate that the factors which constitute the barriers and the enclosed field are not specific to this limited situation but are of quite a general nature.

Patients of Group B (insight into the basic factors of hospitalization, p. 385) find themselves in a field which is not determined by the given place and the present moment and is separated from the outside field by a barrier which is highly structured (Fig. 5). Its structure is such as to provide an opening; certain conditions are stated as definitely warranting the discharge from the institution. These conditions, which the patient sees as not directly dependent on himself, usually imply the passage of a certain time. Therefore, his psychological situation includes the anticipation of the gradual approach to the goal, the locomotion consisting in this case not so much in his own efforts as in merely waiting for the time to pass. As he sees no other alternative, this course does not acquire for him the character of a roundabout-way out of the hospital. Yet the two situations are similar in that they are not completely surrounded by bar-

riers. This characteristic accounts for the absence of strong affect in both cases.

One of the conditions on which the opening of the situation depends is the wider outlook of the patients of this group, their ability to see not only concrete things but also the underlying relations and to see the present in the light of the future. It is not, however, the only condition. Factors may exist which can undermine the patient's belief in his liberation and thus transform the situation temporarily or permanently into a closed form. The results of a permanent transformation of this kind are best shown by the behavior of a small subgroup of Group B (B1) which was omitted from the previous description. These patients, though they have a good insight into their situation and do not struggle in vain against it, show at the same time a strong affectivity. This affectivity differs from that of the primitive group by being less outspoken and much less on the surface. It constitutes the constant background of the patient's behavior and may lead to either exaggerated affective reactions to trifles or—by way of avoiding every opportunity for such outbursts—to an attitude of extreme reserve. The consideration of the patient's prospects for the future reveals the causes of this behavior. He knows from his previous experience, or because he has a correct insight into the circumstances, that his stay in the hospital will last for many years, a period of time which is so long that it is equivalent to an indefinite stay. Thus his psychological situation, though of wider extent and more structured than that of the primitive group, is essentially the same; the field is permanently surrounded by a barrier and this leads to a strong affective tension. The better insight of the patient prevents him from blindly attacking the barriers and, because the tension is constantly dammed up, any slight additional pressure may lead to an affective explosion. This shows that the presence or absence of affectivity is due not to the extent or to the qualitative character of the psychological situation, but to the presence or absence of a barrier surrounding the field.

For the patients of Group C (refusal to accept the reality of the hospital situation, p. 386), the field is determined by the identification with a social group: the enclosed field is constituted by the inferior group of mental patients, the outside field by the group of normal people. Between these groups stands as a barrier the depreciatory opinion of the normal group. This barrier completely surrounds the field, and the outward directed drive of the patient is extremely strong. This situation, though common for all patients of this group, may result in various behaviors. We distinguish the following cases.

A person who has been forced by social opinion to join the inferior group of mental patients may recognize this as a fact, and in his struggle for a position in the group of normals, remain in the plane of reality (Fig. 6). The means which he uses is determined by the character of the barrier. By arguing against the legality of his hospitalization, by deliberately stressing his normal behavior, the patient tries to obtain a practical acknowledgment of his normality, first from his direct social surroundings and eventually also from those who are authorized to discharge

him. The affect arising as a result of the barred situation takes, in this group, the form of indignation. The frequent criticism of the employees serves to place the patient, at least temporarily, above those whose superiority he contests.

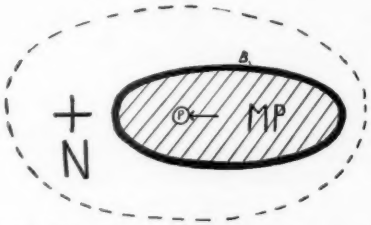

FIG. 6. ILLUSTRATING SITUATION OF PATIENT OF GROUP C WHO REMAINS IN THE PLANE OF REALITY

P = Patient
MP = Group of mental patients
B = Depreciatory social opinion
N = Group of normal people

The person whose behavior is characterized by the denial of his actual position of a mental patient does not remain in the plane of reality. Finding no way out of his actual situation, he flees to unreality (Fig. 7)

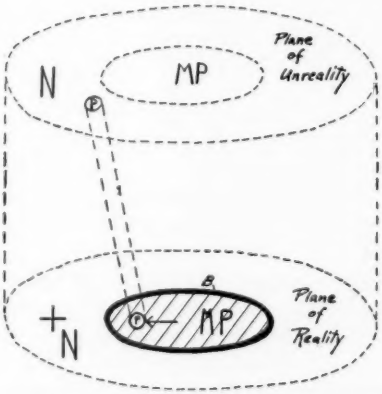

FIG. 7. ILLUSTRATING SITUATION OF PATIENT OF GROUP C WHO FLEES TO UNREALITY

P = Patient
MP = Group of mental patients
B = Depreciatory social opinion
N = Group of normal people

where he is free from all restrictions. As the barriers are weakened in the unreality, the patient, by passing into this plane, succeeds in entering the region which constitutes his goal, that of normal people. Very often

this is the only way for him to escape the unbearable position of social inferiority. The prospect of the termination of the stay in the hospital is not decisive for a member of this group; the discharge does not completely make up for the fact of having been considered insane. Therefore the patient refuses even to enter the situation out of which he sees no possible way, and fleeing into unreality pretends that this situation does not exist. At the same time he does not neglect the reality. He acts in the real situation, but all his actions are determined by the imaginary, as well as by his real, situation. This accounts for the display of theatrical behavior with all its incongruities and contradictions. Even this acting, however, may have a function in reality; by inducing those about him to treat him at least with an appearance of respect, the patient to a certain

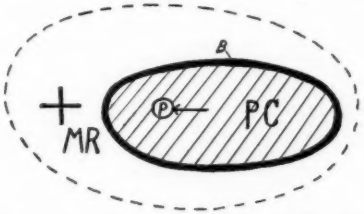

FIG. 8. ILLUSTRATING SITUATION OF PATIENT OF GROUP D WHO HAS PARTIAL INSIGHT INTO HIS CONDITION

P = Patient
PC = Psychotic content
B = The disease
MR = Mental recovery

degree succeeds in changing his real situation for the better. In the cases in which unreality becomes finally predominant, all attempts at changing the real situation disappear and the patient spends his time inactively waiting for some imaginary power to release him.

For the patients of Group D (preoccupation with psychosis, p. 387) the dynamics of the situation are different according to the presence or absence of insight into the abnormality of their new experiences. With patients who have a *partial insight* into their condition, the disease becomes a barrier keeping them in the field which they would like to leave (Fig. 8). It is the only case where the psychosis *as such* is the barrier around the field of the patient—a field constituted largely by the psychotic contents. The observable attempts at leaving this field take the form of demands for help and cure. The inner fight of the patient against the disease does not find any expression in his outward behavior. The barriers inherent to the hospital as such are not attacked by the patient, who is interested in leaving the hospital only as a sequel to getting well. When *no insight* is

present and the patient does not want to leave the field of his psychotic contents, there is no barrier for him around this field.

With patients of Group E (hospital as a place of refuge, p. 389) the theoretical interpretation has to account for the fact that, while these patients give evidence of discontent with their situation they are not anxious to leave the hospital. The barrier in this case lies not only around the hospital situation, but also around the general life situation of the patient (Fig. 9). It is this situation of having no job, no home, of being threatened with imprisonment, which he would like to leave if he could.

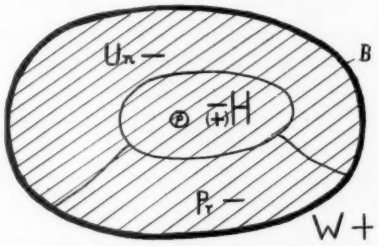

FIG. 9. ILLUSTRATING SITUATION OF PATIENT OF GROUP E FOR WHOM THE HOSPITAL IS A PLACE OF REFUGE

P = Patient
H = Hospitalization
Un = Unemployment
Pr = Prison
B = "Bars of Circumstances"
W = The non-existing "better world"

The negative valence of the surrounding regions may even give the hospital region a positive valence, and that is why the patient makes no attempts to leave the hospital. The regions which have an absolute positive valence lie outside of the totality of the possible life situations; they belong to a non-existent world in which the environment would be more favorable and the patient 'a better man.' By comparison with this ideal, all possible situations, including that of the hospital, are negative. The situation of this group is equivalent to that of the Groups A-D, because there is no way out of the negative region which in this case is identical with the entire field of reality. This group is even at a disadvantage because no attack seems possible against the 'bars of circumstance.' The all-inclusiveness and firmness of the barriers is probably due to the fact that they are so intimately connected with the person himself.[6]

[6] For members of Group F (the narrowed situation, p. 391) the general hospital situation cannot be said to have or not to have barriers around it, because this general situation does not seem to exist. The structure and the contents of the field change continually, and these changes do not seem to follow a definite principle.

Practical Implications

In the discussion of the practical implications of our findings for the handling of the patients, it should be kept in mind that the ultimate aim of the hospital is to bring about a state of affairs resulting in the discharge of the patient. Only in the case of failure to achieve this aim does the task of keeping the patient in the institution permanently and of taking care of him in a most appropriate way acquire primary importance.

The outward directed drive of the patient is a psychological factor which may be utilized in the process of bringing him to a stage where he can actually be discharged. Therefore, the presence of this drive should be highly evaluated from the standpoint of the ultimate aim of the hospital, even though it may be very inconvenient for the purpose of making the patient accept the temporary stay in the hospital.

From this point of view, Groups A, B, and C, which display the strongest outward-directed drive, may be considered the most hopeful ones. This drive has to be directed and utilized. This can be attempted by purely 'educational' means. The situation must be transformed into a relatively open one, either by strengthening the belief in the termination of the hospital stay wherever the facts do not contradict it, or else by helping the patient to find a roundabout-way, *e.g.* in pointing out which type of behavior will bring about his discharge. This change of situation is favored by a composed and quiet mood of the patient, and therefore everything which may become an additional cause of affect should be avoided. The transformation of the situation is the easiest for Groups A and B. In the case of Group C the specific character of the psychological field makes the opening of the situation nearly impossible, after it has been once closed. 'Once a mental patient always a mental patient' is the attitude of this group. This danger of a permanent psychological enclosure of the patient in the situation of social inferiority is very serious and should always be taken into account. His refusal to acknowledge his position as a regular mental patient has to be interpreted as a useful defense and should not be rejected as pretentiousness or as annoying and ridiculous fussing. This does not imply an actual granting of exceptional privileges to the patient; on the contrary, the actual demands of his real situation should always be stressed, but this should be done preferably by reference to the routine of the hospital without stressing the implication of mental disease.

The patients of Groups A, B, and C are those for whom the information about the hospital and its rules and requirements is of utmost importance, as it gives them means at hand to fulfill their final purpose, *i.e.*, to

leave the institution. All 'educational' measures of the management, such as the informational letter, radio talks, and suggestions of personnel, are most successful with these groups.

The attitudes of the Groups D and E are negative with respect to the basic purpose of the hospital, in so far as the wish to leave the hospital is either weak or completely lacking. Therefore they are little helped by general suggestions to this purpose before the outward-directed drive is sufficiently strengthened. This, however, can be done only as a sequel to the removal of the barriers of the patient's specific situation. For Group D, the barrier when it exists, is the psychosis as such. This is the only group which actually expects from the hospital treatment and cure. What these patients want is somebody's help in the understanding and disentangling of their mental troubles, and it is incumbent upon the psychiatrist to give this help and also to strengthen the desire for recovery or to awaken it when it is lacking. Psychotherapy is the subjective requirement of many of these patients. For Group E, the barrier is constituted by the unfavorable life circumstances. As long as the patient feels that he has nowhere to go, he either has no desire to leave the hospital or else this desire cannot become effective. What he wants is a complete change of his life situation, which can be attempted in some cases through the mediation of the hospital. Thus Group E is the one for which most can be done by the social worker. As no definite attitude towards the hospital could be stated in Groups F and F_a, no directions for specific handling can be given. The problems of medical and custodial care acquire primary importance in these cases.

The differential treatment of the patients, which takes into account their specific psyschological hospital situation, may complicate the routine, but the results should fully compensate for the expenditure of time and effort.[7]

APPENDIX

Analysis of the Groups with Regard to Institutional Status, Educational Level, Sex, Age, and Diagnosis

The patients were assigned to one of the seven groups described above on the basis of their psychological situation only (Group F and F_a are considered here as two separate groups). No consideration was paid to such characteristics as diagnosis,

[7] Such treatment is obviously not a substitute for any physical therapy or psychotherapy called for by the nature of the disease. It may, however, prove a good preparation and a valuable addition to the specific therapy when it is available.

institutional status, or age. Tables II to VI show the constitution of the different groups with regard to these characteristics.

Table II shows the relation between the psychological situation and the institutional status. The ratio of first admissions to readmissions and transfers approximates 3:2 for the total number of patients. (The transfers from Boston Psychopathic Hospital occupy an intermediate position with regard to the familiarity with the Hospital routine and are not considered here.) Groups A, D, F, and F_a show a deviation from the ratio in favor of first admissions, Groups B and E in favor of

TABLE II

COMPOSITION OF THE PSYCHOLOGICAL GROUPS WITH REGARD TO INSTITUTIONAL STATUS

Institutional status	Group							Total
	A	B	C	D	E	F	F_a	
First admission	13	11	5	4	3	6	11	53
Readmission and transfer	2	16	5	0	7	1	2	33
Transfer from Boston Psychopathic Hospital	2	2	1	4	0	4	1	14
Total	17	29	11	8	10	11	14	100

readmissions. Although the figures are too small to be of statistical significance, the direction of the deviation is in conformity with the general characteristics of the respective groups. The lack of insight, the primitive affective behavior of Group A, is partly accounted for by the predominance of first admissions in this group. On the other hand, for the readmitted or transferred patients, the more quiet and controlled behavior of Group B is typical, the previous hospitalization followed by discharge being one of the possible causes of better insight into the hospital situation. In Group E the familiarity with the hospital deprives it of its frightening character and

TABLE III

COMPOSITION OF THE PSYCHOLOGICAL GROUPS WITH REGARD TO EDUCATIONAL LEVEL

Educational level	Group							Total
	A	B	C	D	E	F	F_a	
Less than 8 grades	6	3	1	0	1	3	6	20
8 grades	8	20	3	2	3	7	5	48
More than 8 grades	3	6	7	6	6	1	3	32
Total	17	29	11	8	10	11	14	100

enables the patient to choose or accept it as a possible refuge. In other groups the connection with the institutional status is less direct. The absolute prevalence of first admissions in Group D is due to the fact that an intensive preoccupation with the psychotic contents combined with partial insight is mostly encountered during the first onset of a psychosis. The prevalence of the first admissions in Groups F and F_a is accounted for by the strong participation in both of these groups of patients in a state of disturbance due to the acute onset of the psychosis, and of patients, who on being admitted to a state hospital are not likely to be discharged from this

or any other institution to which they have been transferred, and therefore seldom figure as readmissions; *e.g.* senile and mentally deficient patients.

The differences in the educational level of the psychological groups are shown in Table III. By comparison with the distribution of the total group over the lower, middle, and higher levels (ratio about 2:5:3), the 'primitive' group A shows a shift of the ratio towards the lower end of the scale, the 'reasonable' Group B a marked concentration in the middle, the 'ambitious' Group C and the 'philosophical' Group D, a strong shift towards the upper end. In other groups the relationship between

TABLE IV

COMPOSITION OF THE PSYCHOLOGICAL GROUPS WITH REGARD TO SEX

Sex	Group							Total
	A	B	C	D	E	F	F_a	
Male	6	18	1	8	6	6	5	50
Female	11	11	10	0	4	5	9	50
Total	17	29	11	8	10	11	14	100

the educational level and the psychological characteristics of the group is not so obvious, the shift towards the lower end being accounted for in Group F by the inclusion in it of the mentally deficient, in Group F_a—by the inclusion of a number of very old persons, who have on the average a lower educational level.

Table IV shows the participation of men and women in different psychological groups. Men prevail in Groups B and D, women in Groups A and C (and Group F_a). In the case of Groups A and B this different participation of the sexes seems to be typical. Women of those social classes to which most of our patients belong,

TABLE V

COMPOSITION OF THE PSYCHOLOGICAL GROUPS WITH REGARD TO AGE

Age	Group							Total
	A	B	C	D	E	F	F_a	
Under 30 yr.	4	8	2	8	3	7	2	34
30–45 yr.	10	7	0	0	1	2	3	23
45–60 yr.	3	12	6	0	3	1	3	28
Over 60 yr.	0	2	3	0	3	1	6	15
Total	17	29	11	8	10	11	14	100

being often limited to the narrow sphere of home life, have more difficulty in orienting themselves in a new situation having a quite different structure. It should be noted that of 11 women who displayed the insightful behavior of Group B, 8 were readmissions and of the remaining 3, 2 had passed through similar institutions, while of the 18 men, 8 were actually first admissions. The better orientation displayed by many men even on the first admission was gained by women only as a result of previous experience in a hospital.

Table V gives the distribution of patients of different groups according to age. This distribution is consistent with the diagnostic constitution of the groups and

with the distribution according to the institutioned status, and reveals nothing new of significance.

Table VI gives the distribution of patients of all seven groups with regard to diagnosis. The table shows no complete correlation between the psychological situation and the diagnosis. Only in a few instances are there certain affinities between the psychological and the diagnostic groups. In the first place there is no overlapping whatever between the largest diagnostic and the largest psychological group—there are no dementia praecox patients in Group B. This group, on the other hand, contains more than half of the next largest diagnostic group, that of patients without

TABLE VI

COMPOSITION OF THE PSYCHOLOGICAL GROUPS WITH REGARD TO DIAGNOSIS

Diagnosis	Group							Total
	A	B	C	D	E	F	F_a	
Senile psychoses			1		1		3	5
Psychoses with cerebral arteriosclerosis		1			1		3	5
General paralysis	2	1		1		3		7
Psychoses with other brain or nervous diseases		1			1			2
Alcoholic psychoses	2	3			1	1	1	8
Psychoses due to drugs and other exogenous toxins	1	1						2
Psychosis with somatic diseases	2	1	1				1	5
Manic depressive								
(a) Manic type		3	2	1				6
(b) Depressive type	2	2					1	5
Involutional melancholia					1			1
Dementia praecox	3		3	6		3	4	19
Paranoia and paranoid conditions	1	3			1			5
Psychoneuroses and neuroses	1	1			2		1	5
Psychoses with psychopathic personality		2			1			3
Psychoses with mental deficiency		1				3		4
Undiagnosed psychoses			1					1
Without psychosis	3	9	3		1	1		17
Total	17	29	11	8	10	11	14	100

psychosis. Both facts show this group to be the 'normal' group *par excellence*. Group D is the direct opposite of Group B. It contains no normals and is largely composed of the young dementia praecox patients with the acute first onset of the psychosis. In other psychological groups there is no significant prevalence of one diagnostic group over the others. The second largest, Group A, especially extends over a wide range of diagnoses.

The knowledge of the patient's psychological hospital situation is clearly valuable for the understanding of many aspects of his behavior, and leads to certain practical applications. The fact that this situation is largely independent of the diagnostic group to which the patient belongs emphasizes once more the necessity of psychological analysis in directions other than purely diagnostical.

THE CHARACTER AND EXTENT OF TRANSFER IN RETROACTIVE INHIBITION: DISPARATE SERIAL LISTS

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Degree of retroactive inhibition has been found to be in part a function of the relations between the original and interpolated activities. This strongly but indirectly supports a transfer theory of the phenomenon according to which the inhibitory effects of the interpolated activity are a result of some non-perseverative interaction between this and the original activity. The transfer theory, however, lacks precision of statement and the evidence for the transfer in retroaction would be strengthened by a direct measurement of what is transferred. The theory does not, for example, state whether the transfer is from original activity to interpolated, from interpolated to original, or in both directions; nor does it state whether the temporal locus of the transfer is during the interpolated activity itself, at the time of the recall of the original material, or at both times. The experimental data do not moreover, show, and the theory does not specify, whether the implied transfer is overt or implicit.

The general problem of the present experiment was to seek for evidence of overt transfer under conditions which would permit it to become apparent, if present, and thereby to define more clearly the transfer theory. If overt transfer occurs under the conditions of the retroactive inhibition experiment, it should appear when learning is by the method of complete presentation and when both learning and retention are tested by written recalls. We have, therefore, gathered data by this method and have analyzed them to discover whether in the recalls there is evidence of a transposition of items from one list to another or of any kind of inter-list confusion. In Experiment I we have employed naïve *Ss* and have examined the objective recalls. In Experiment II we have taken both the objective recalls and the verbal reports of more trained *Ss*.

METHOD AND PROCEDURE

Experiment I. In both this and the following experiment the usual retroaction procedure, which involves the comparison of measures of retention after rest and work conditions, has been employed. One group of *Ss* served under rest and another group under work. The original lists under the rest condition and the original

* Accepted for publication June 8, 1934.

and interpolated lists under the work condition consisted of lists of 15 adjectives. Learning was by the method of complete presentation and retention was tested by written recalls. Two minutes were in each case allowed for learning and the instructions were designed to motivate the *S*s and to produce learning with intent to retain. At the end of the 2-min. study-period, the *S*s, under instructions given before learning began, wrote all of the items which they could recall. They were instructed to try to recall in serial order. Four minutes were allowed for each recall.

Under both *rest* and *work* conditions 15 min. elapsed between the end of this recall and a second or delayed recall conducted in the same way. Under the rest condition the 15-min. interval was filled by a series of successive temporal judgments by the *S*s.[1] Judging the length of temporal intervals is, of course, not rest in the sense of absence of mental activity, but it does represent freedom from any formal learning. With untrained *S*s it is necessary to introduce during the rest interval some non-learning activity which will reduce the probability of rehearsal. The interest of the *S*s in the judgments, together with the close observation of the *S*s' behavior by E, leads us to believe that rehearsal was at a minimum. A third recall was taken 11 days later in the same experimental setting.

Under the work condition the *S*s turned, immediately after the first recall of the original list, to the study of a second or interpolated list for 2 min., and then to a written recall of it for 4 min. This interpolated list contained 15 adjectives each of which was a synonym[2] of the adjective in the corresponding position in the original list. Interpolated synonyms have been used because they are known to produce relatively large amounts of inhibition and because they offer a favorable condition for the appearance of overt transfer if it occurs. The time devoted to study and recall was 6 min. The remainder of the 15 min. interval was filled by temporal judgments as in the rest condition. At the end of the interval a second recall of the original list was taken and, after 11 days, a third.

We have, then, for comparison, two conditions which differ only in that, under the work condition, an interpolated list of synonyms was learned and recalled. The lists used as original learning materials were the same in the two conditions and the same experimental procedure, except for the variable of the interpolation, was rigidly adhered to.

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[1] The intervals to be judged were bounded by a tap made by *E* with a steel rod and were formally unfilled. Immediately after the second bounding signal *S* recorded his judgment of the length of the interval. The intervals varied in length from 1 sec. to 62 sec., longer and shorter ones being distributed at random. The *S*s were permitted to count or to use any device to promote accuracy. At the end of every 5 intervals judged, *E* read the actual elapsed times and permitted a few seconds for a comparison by the *S*s of times and judgments. This served to maintain motivation. The methods used were dictated by the requirements of the particular experiment and no use is made of the obtained judgments.

[2] The lists are as follows: Original: (1) mixed, perfect, exact, serene, filthy, valued, insane, complete, pensive, cruel, fearless, devout, vacant, soiled, famous; (2) dreadful, profuse, local, crafty, angry, mulish, infirm, ample, gloomy, timid, peaceful, sacred, fitting, cursed, urgent. Interpolated (synonyms of the words of the original list: (1a) jumbled, faultless, precise, placid, dirty, prized, crazy, entire, thoughtful, brutal, daring, pious, empty, stained, noted; (2a) frightful, lavish, nearby, cunning, wrathful, stubborn, feeble, plenty, dismal, bashful, quiet, divine, proper, damned, pressing.

The experiment under both conditions was conducted as a group experiment. The variable of copying by one S from another was controlled by careful supervision and by the use of 2 lists in each part of the experiment. One list was learned by students occupying the odd seats in the classroom and the other by students occupying the even seats. The adjectives were mimeographed vertically in the center of a sheet of paper 8½ x 11 in. At the beginning of the experiment with each group every *S* was given all of the material which he would need, including blank sheets for the writing of the recalls, clipped together in order, with the proper designation written on the back of each sheet. At each successive step *S* had only to place the sheet on which he had been working face down at the bottom of the packet of material, turn over the next sheet at the proper signal, and proceed. In this way it was possible to follow exactly the time schedule of the experiment. At the completion of the second recall the *Ss* were given detailed instructions against rehearsal.

The *Ss* were students in two sections of General Psychology meeting at consecutive hours on the same days. Complete records are available for 69 *Ss* under the rest condition and for 59 under work. The *Ss* were unacquainted with experiments upon learning and had not reached the topic of learning in the course.

The recalls have been scored in terms of number of adjectives correct regardless of order. The *Ss* had been instructed to learn and recall in order and the deviations were relatively few. Percentage of retroactive inhibition has been computed in two different ways. In Method I, the percentages which the delayed recalls are of the immediate recalls have been found and relative retroactive inhibition has been found in terms of the percentage which the difference between recall under rest and work is of recall under rest. In Method II, relative inhibition has been found by substracting the delayed recall under work directly from that under rest and dividing by the latter. This method disregards differences between the two conditions with respect to degree of learning. The recalls were analyzed further in ways which can best be described in the context of the results.

Experiment II. This experiment is a duplicate of Experiment I with respect to all but the following points. (A) There was no delayed recall after 11 days. (B) Semi-trained *Ss*, who had served previously in several experiments in learning and who had taken several courses in psychology were used. (C) The experiments were conducted individually. (D) Both conditions were gone through by each *S* and at a single sitting. (E) At the conclusion of the work condition the *Ss*, who had not been informed of the specific purpose of the experiment, were asked to report upon the presence or absence of interference and upon matters relevant thereto. They were asked (1) whether any similarity was noticed between the original and interpolated list; (2) if any were noticed, of what kind it was; (3) whether any words were recalled, during the writing of the recall of the interpolated list or the delayed recall of the original list, which were rejected and, if so, what the words were and why they were rejected and not written down; and (4) whether any interference was thought to have occurred between the two lists. These questions were asked of all *Ss*, and others were added, as necessary, to make clear the reports. The *Ss* gave more detailed information than the formal questions alone indicate.

Each of the writers acted as *E* for 10 *Ss*. Method was carefully rehearsed in advance to insure constancy. The *Ss* were taken through the rest and work conditions in a counterbalanced practice order.

RESULTS

Experiment I. In this experiment the *S*s under each condition were divided into two groups and a different original list was used with each group. Since this fractionation of the total group offers an internal check upon the consistency of the results, we shall present the results for the two groups separately. The means and their sigmas, the percentages which the delayed recalls are of the corresponding immediate recalls, and the percentages of retroactive inhibition are shown in Table I.

TABLE I

MEANS AND SIGMAS OF THE MEANS OF IMMEDIATE AND DELAYED RECALLS, PERCENTAGES RECALLED AND PERCENTAGES OF RETROACTIVE INHIBITION

	Group 1		Group 2	
	Rest (32 Ss)	Work (28 Ss)	Rest (37 Ss)	Work (31 Ss)
Recall 1 (Immed.)	9.91 ± .50	10.93 ± .46	10.22 ± .45	9.87 ± .50
Recall 2 (15 min.)	8.84 ± .63	6.79 ± .72	9.35 ± .57	6.06 ± .60
% Recalled	89.2	62.1	91.5	61.4
% R. I., Method I	30.4		32.9	
% R. I., Method II	23.2		35.2	
Recall 3 (11 days)	4.53 ± .61	3.00 ± .36	5.89 ± .60	2.68 ± .40
% Recalled	45.7	27.4	57.6	27.1
% R. I., Method I	40.0		52.9	
% R. I., Method II	33.8		54.5	

It is necessary to inquire first whether the immediate recalls under rest and work are sufficiently close together in amount to justify comparisons between delayed recalls under the two conditions. The means differ, but by amounts which are statistically unreliable, and the direction of the difference in Group I is reversed in Group II. It may be concluded that the inhibition obtained is not importantly ascribable to differences in degree of learning as measured by immediate recall.[3]

The percentages recalled after 15 min. by the two groups are very nearly the same under the corresponding conditions and the percentages of inhibition are, especially when found by Method I, close together in amount. The percentages recalled after 11 days are practically equal under work but differ by 10% under rest. Group II shows the greater amount of recall under rest and, also, a larger percentage of inhibition. The important fact, however, is the increase shown by both groups in percentage of inhibition from 15 min. to 11 days. This is in accord with the results of an

[3] The immediate recalls of the interpolated lists are 11.54 ± .53 and 10.35 ± .50 for Groups I and II, respectively. The difference has a critical ratio of 1.63.

analogous experiment with poetry as material and with recall after 15 min. and 7 days.[4]

The data upon amounts of inhibition are significant in the context of the present problem in that they make certain that the experimental conditions have produced a considerable amount and that, therefore, the recalls are a valid place in which to seek for evidences of overt transfer.

We shall proceed, then, to the question of the overt transfer. This has been looked for by examining the written recalls of both original and interpolated lists and by recording the cases of words which were correct for one list but which appear incorrectly in the other. A word which was learned as a part of the original list but which appears in the immediate recall of the interpolated list, or one which was learned as part of the interpolated list but which appears in a delayed recall of the original list, constitutes a case of overt transfer. The number of such cases is given in Table II, together with the number and percentage of *Ss* showing such transfer.

TABLE II

CASES OF OVERT TRANSFER OF ADJECTIVES FROM ONE LIST TO ANOTHER

Direction of Transfer	Group 1			Group 2		
	Words	Subjects	% Ss	Words	Subjects	% Ss
Original to Interpolated	5	3	10.7	6	5	16.1
Interpolated to Original						
Recall 2	19	9	32.1	21	15	48.4
Recall 3	17	14	50.0	23	14	45.2

The table presents a picture of at least some overt transfer in both directions. Much more occurs, however, from interpolated list to original than in the reverse direction. Three times as many *Ss* show transfer at Recall 2 from interpolated to original as from original to interpolated and the number of words transferred is more than 3 times as large. Roughly similar relations obtain at Recall 3. There is no clear increase in overt transfer from Recall 2 to Recall 3. There is throughout an average of more than a single word per *S* showing transfer. It is clear from these data that overt transfer does occur. The *Ss* accepted the transferred words as correct and recorded them as part of the list which they were recalling. To the extent of the occurrence of the transfer, therefore, there is direct evidence that a part of the decrement from interpolated learning is a transfer phenomenon. This transfer is positive in the sense that items are actually carried over, though un-

[4] J. A. McGeoch and F. McKinney, Retroactive inhibition in the learning of poetry, this JOURNAL, 46, 1934, 19-33. The significance of the increase in amount of inhibition has been discussed in that paper.

intentionally, from one list to the other; it is negative in that it reduces the number of correctly recalled words.

There are certain additional facts of importance. All of the 19 words which overtly transferred from the interpolated to the original list in Recall 2 by Group I were correctly recalled in the recall of the interpolated list. Of the 17 of these words which transferred at the third recall 16 had been correctly recalled in their own list. The corresponding figures for Group II are 19 out of 21 and 22 out of 23. The words which transferred are, thus, almost all words which had been learned to the threshold or above as part of the interpolated list. In the same way, the few words which transferred from original to interpolated list had been recalled correctly at the recall of their own lists but, with few exceptions, did not reappear in the second and third recalls of their own lists after having been incorrectly recalled as part of the interpolated list. This fact supports the interpretation of the cases of overt transfer as being productive of inhibition.

The amount of overt transfer which has been found cannot be taken to represent the total amount of interference resulting from transfer. It represents only the transfer effects which were sufficiently powerful to become overt. There were in the written recalls 15 cases of words from other lists which had been written down and scratched out. This points to the existence of an initial confusion resulting from transfer and suggests, as do the reports of Experiment II, that there is a considerable amount of transfer which may lead to forgetting but which does not gain expression as a wrong item which is accepted by the S as correct. There may be, too, transfer effects which gain no overt expression other than in the blocking of the recall of other items. The existence of as much overt transfer as we have found can account for a portion of the total inhibition; it is possible that transfer effects which are not accepted as correct and those which gain no overt or reportable expression may account for much, at least, of the remainder.

The question immediately arises to what extent the amount of retroactive inhibition reported in Table I may be referred to the overt transfer. We may obtain one answer to this question by finding the mean number of words forgotten under work and by finding the percentage which the mean of the transferred words is of this number. We shall call this Method A. At Recall 2 the mean number of words transferred from interpolated list to original is 0.68 for each group. A mean of 4.14 words was lost under the work condition in Group 1 from Recall 1 to Recall 2 and a mean of 3.81 in Group 2. The mean numbers of words transferred are 16.4% and 17.8% respectively, of these losses. If, now we subtract from 4.14 and 3.81 the amounts lost as a result of other factors than the interpolated

learning, that is, the number of words forgotten during rest, and find the percentage the mean number of words transferred is of these in the case of each group, we obtain a measure of the loss which may be ascribed to transfer. Subtracting 1.07 and 0.87, the mean amounts lost under the rest condition, we get 3.07 and 2.94, and dividing these into 0.68 we get 22.1% and 23.1%. When the data for Recall 3 are treated in the same way, we find mean losses of 7.93 and 7.19 under the two work conditions. The mean numbers of words transferred from interpolated to original list at Recall 3 are 0.61 and 0.74, which are 07.7% and 10.3%, respectively of the mean losses. Subtracting the losses under rest and dividing as above, we obtain 23.9% and 25.9%, or roughly one-fourth, of the loss which may be ascribed to overt transfer from interpolated list to original.

If we add in the cases of transfer from original to interpolated list, the percentages increase. The mean cases of overt transfer then become 0.86 and 0.87 at Recall 2, 0.78 and 0.94 at Recall 3, and the percentages which these means are of the net loss (rest minus work) become 28.0 and 29.6 at Recall 2 and 30.6 and 32.9 at Recall 3.

It must be remembered, however, that these percentages have been obtained by a different operation than that commonly used in computing percentage of inhibition and shown as Method I in Table I. They represent the percentages of the net loss which may be referred to transfer but they take no account of the relative size of the initial recalls from which the second and third recalls have been subtracted to obtain the net losses. The means at Recall 1 do not vary greatly but they vary enough to forbid a direct translation of the percentages under discussion into percentages of inhibition referable to overt transfer. The means at Recall 1 are not, however, different by statistically reliable amounts. If we assume, for the moment, that they are equal, we may accept the results by this method as approximations.

The same is true when we compute percentages of retroactive inhibition directly from the means at Recall 2. The percentages computed from these figures are 23.2 and 35.2. If we assume that without transfer in either direction Recall 2 under work would have been increased by the amount of the transfer, that is by 0.86 and 0.87, and if we compute the percentages of inhibition with these amounts added to the recalls under work, the former become 13.5 and 25.9. Subtracting these from 23.2 and 35.2 we have 09.7 and 09.3, the gross percentages which may be referred to transfer. These figures are 41.8 and 26.4% of the percentages of inhibition computed without regard to transfer and represent the relative amounts ascribable to transfer by this method of computation. Similar operations at Recall 3 give

relative amounts, or percentages, of 51.2 and 29.4 which may be ascribed to transfer.

We have summarized the results of these operations, when the overt transfer in both directions is included in the computations, in Table III. It should be repeated that by Method A we have estimated the amount of inhibition which may be referred to overt transfer by computing in terms of the net loss, when the number of transferred items is divided by loss under work minus that under rest. By Method B we have estimated in terms of the proportion of the inhibition, as computed in percentages which Recalls 2 and 3 under work are of Recalls 2 and 3 under rest, which is ascribable to transfer. In both cases it has been assumed that the degrees of learning, as measured by Recalls 1, are equal. This assumption is only approximately justified by the measures of Recall 1, but it is sufficiently

TABLE III

ESTIMATES OF THE PERCENTAGE OF THE INHIBITION WHICH MAY BE ASCRIBED TO TRANSFER IN BOTH DIRECTIONS

	Group 1		Group 2	
	Recall 2	Recall 3	Recall 2	Recall 3
Method A	28.0	30.6	29.6	32.9
Method B	41.8	51.2	26.4	29.4

justified to make the estimates trustworthy within reasonably narrow limits of variation. The estimates by Method B are directly interpretable in the same terms as the inhibition computed by Method II, while those of Method A are not directly analogous to the inhibition computed by Method I. The implications of Methods A and B are, however, the same.

It may be concluded that at least a fourth of the decrement from interpolated learning may be with caution ascribed to a transfer of items from original list to interpolated and from interpolated list to original, and that inhibition from transfer in the latter direction is much greater than the inhibition from transfer in the former.

We may inquire also, whether the interpolated learning affected recall by producing an increase in other errors than the inclusion of words from the interpolated list. In order to arrive at an answer the errors at each of the recalls under each condition have been classified under 3 headings: (1) *substitutions* of synonyms other than those which appeared in another list, where a second list had been learned, that is, under work; (2) *inclusions* of new words which were not directly synonyms, some of which were clearly similar to the correct words in sound or meaningful context or in some other way, but all of which were items completely outside any of the lists

used with the given group; and (3) *transpositions* of a correct word to an incorrect serial position. The results of this classification for each group and under each condition at all 3 recalls appear in Table IV in terms of the total number of errors.

The data of this table show certain facts of interest. Substitution of a synonym other than the one which appeared in the original list is infrequent but occurs consistently in that at least 2 cases of it are found at every recall. It does not vary from rest to work condition. There is a much larger number of inclusions of new words. The inclusions within a given condition are roughly equal at the first two recalls and increase sharply

TABLE IV

FREQUENCIES OF THREE KINDS OF ERRORS AT EACH RECALL

	Group 1						Group 2					
	Rest			Work			Rest			Work		
	No. of Recall			No. of Recall			No. of Recall			No. of Recall		
	1	2	3	1	2	3	1	2	3	1	2	3
Substitutions (Syns.)	5	3	3	3	4	5	3	2	3	4	4	2
Inclusions (New Words)	18	14	22	4	8	45	8	8	25	9	10	25
Transpositions	11	13	16	19	9	7	19	20	26	17	15	11
Total	34	30	41	26	21	57	30	30	54	30	29	38

from the second recall to the third. They do not vary consistently from condition to condition: at the first two recalls they are largest under the rest condition with Group 1, but this is not supported by the results of Group 2, nor do the results at Recall 3 show consistency of relative increase. We must conclude that, while inclusions are relatively frequent, they do not vary uniformly from rest to work. The interpolated learning has, therefore, produced no consistent increase in errors of inclusion other than that due to transfer from one list to the other. Transpositions yield only one consistent tendency. They increase from Recall 1 to Recall 3 under rest and decrease under work. In the work condition the number recalled is much less than under rest and there are, therefore, fewer words to transpose. This will account in part, although not entirely, for the decrease under work. The total number of errors shows no regular change either with interval or condition.

It is worth recording, although the fact is only indirectly related to our present problem, that a number of items occurred in the second and third recalls but not in the first. These items may be classified as cases of reminiscence. In Group I the numbers at the two delayed recalls were 11 and 18 under rest, 5 and 21 under work. In Group II they were 19 and 22, 12

and 12, respectively. There is, in three out of the four comparisons, an increase from the second to the third recall. The increase with interval is greater under the work condition in Group I but there is no increase at all under work in Group II. There are no other more regular differences between rest and work. We cannot conclude that the interpolated learning has had any clear influence upon reminiscence as here measured.

Experiment II. We shall turn now to the results of this experiment and shall reserve until later a discussion of the implications of those of Experiment I. The mean recall scores, the percentages of recall after 15 min., and the percentages of retroactive inhibition are given in Table V. The initial recalls are somewhat larger than those of Experiment I but the percentages of inhibition are of the same order of magnitude. The inhibition found by

TABLE V

MEANS AND SIGMAS OF THE MEANS OF IMMEDIATE AND DELAYED RECALLS, PERCENTAGES RECALLED AND PERCENTAGES OF RETROACTIVE INHIBITION

	Experimenter 1		Experimenter 2	
	Rest	Work	Rest	Work
Recall 1	12.0 ± .20	11.9 ± .92	11.4 ± .78	11.6 ± .82
Recall 2	8.6 ± 1.29	5.4 ± .66	10.2 ± 1.02	8.0 ± 1.46
% Recalled	71.6	45.3	89.5	68.9
% R. I., Method I	36.7		23.0	
% R. I., Method II	37.2		21.6	

Experimenter 1 is somewhat larger than that found by Experimenter 2. This is probably in part a function of the small samples compared and in part a result of the fact that most of the *S*s used by Experimenter 2 were more familiar with experiments in retroactive inhibition and were able to recognize the kind of experiment in which they were serving. This might be expected to produce a set which would minimize degree of inhibition.[5] The data show that sufficient inhibition has been produced to warrant us in searching in the reports of the *S*s for evidence of overt transfer.

We shall turn, then, to the reports of the *S*s. Fourteen of the 20 reported that they noticed similarity between the two lists under the work condition and all of the 14 noticed that most or all of the words in the two lists were synonymous. During the second recall of the original list words from the interpolated list frequently occurred to the *S*s but were discarded as not belonging to the original list. Several reported that a word from the interpolated list was recalled and was recognized as being from that list, whereupon the correct word would be found by thinking of synonyms for the recalled word until one recognized as correct had been found. One *S*

[5] See O. P. Lester, Mental set in relation to retroactive inhibition, *J. Exper. Psychol.*, 15, 1932, 681-699.

was unable to prevent the recall of the interpolated list while he was under instruction to recall the original, and used the former as an aid to the recall of the latter. Several *Ss* were unable to say with certainty how many words from the interpolated list were recalled and were discarded, but from the best estimates they could make the mean number is 3.2. Some of the *Ss* were able to report exactly which incorrect words had been recalled and discarded and on the basis of their precise information and of the apparent conservatism of the estimates of those who were unable to be as precise, it seems that the mean of 3.2 for all *Ss* is approximately accurate. The mean for the 12 *Ss* who gave apparently precise statements of the number is 4.2.

No words from the original list were reported as having been recalled and discarded during the recall of the interpolated list. This may have been in part a result of the fact that the reports were not given until after the second recall of the original list. The *Ss* may have forgotten during the interval the occurrence of some words from the original list. The fact, however, that in Experiment I only a few words transferred overtly from original list to interpolated permits the inference that the number to be forgotten in the present case was not large.

Eight *Ss* show a total of 14 cases of overt transfer from interpolated list to original in the second recall of the latter, or a mean for the whole group of 0.7, which is very close to the mean of 0.68 found in Experiment I. The mean loss under work in the two groups combined is 5.1, and the mean number of words overtly transferred is 13.7% of this loss. The percentage of inhibition, as estimated by Method A, which may be ascribed to transfer is 25.1. The sum of the mean scores at Recall 1, when the two groups are combined, is 23.4 under rest and 23.5 under work, so that we may consider degree of learning to be a constant. The percentage of inhibition computed directly from the combined Recalls 2 is 28.7.[6] When by Method B, overt transfer is added to the mean under work, this percentage decreases to 21.3 and the difference of 7.4, divided by 28.7, gives a percentage of 25.8 which may be referred to overt transfer. This percentage, together with that obtained by Method I, is of the same order of magnitude as those of Experiment I.

The transferred words without exception *took the place of the correct words* and thereby decreased the amount of the correct recall. The protocols show, as we have seen, a considerable amount of recall of words from the interpolated list which are discarded. These do not overtly operate to produce inhibition, but the fact that they occur and that some are not dis-

[6] The mean percentage of inhibition by Method II, when the percentages are found separately and added, is 29.7. We shall use, however, the figure resulting from the direct summation of the means.

carded but are accepted as correct and are written down, suggests that their occurrence may have produced more inhibition than is shown by the overt transfer alone. Ten of the *Ss* definitely reported that the interpolated list was confusing, largely by virtue of the similarity of the constituent words. The others were either uncertain or, as in the case of two *Ss*, said that the interpolation had aided the recall of the original list by permitting association from an interpolated word which could be recalled to one in the original list which at first could not be.

Discussion

The two experiments agree in finding considerable amounts of overt transfer of items from one list to another under the work conditions. These transferred items take the place of the correct items and, therefore, decrease the amount of correct recall. The two methods of estimate used place the total amount of interference thus produced at somewhat more than a quarter of the total amount, and we may take 25% as representing a conservative estimate. The results provide direct evidence for transfer as a major condition of retroactive inhibition. Since the transfer is much greater from interpolated list to original, although some occurs in the reverse direction, they specify the direction of transfer and the relative importance of the two directions. They show, that is, that the interference resulting from transfer is a two-way interference, but that the interpolated-to-original direction is the more important under these conditions.

The fact that there is such actual transfer and that the *Ss* report the recall of items from the interpolated list which are recognized as wrong and discarded strongly imply that the temporal locus of the interference is at the time of recall. This implication is supported by the fact that percentage of inhibition is larger after 11 days than after 15 min. The relatively weaker or poorer organization of the lists after the longer interval, as a result of the inhibitory effects of intercurrent experience,[7] may well have rendered them more susceptible to interference effects from transfer. The implication is, moreover, in agreement with that of a previous experiment with poetry performed by an analogous procedure.[8]

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[7] See J. G. Jenkins and K. M. Dallenbach, Obliviscence during sleep and waking, this JOURNAL, 35, 1924, 605-612; E. B. van Ormer, Retention after intervals of sleep and of waking, *Arch. of Psychol.*, 21, 1932, (no. 137), 49; and Sleep and retention, *Psychol. Bull.*, 30, 1933, 415-439.

[8] McGeoch and McKinney, *op. cit.* Cf. also, the discussion of the question of the temporal locus of transfer by J. A. McGeoch, Studies in retroactive inhibition: I. The temporal course of the inhibitory effects of interpolated learning. *J. Gen. Psychol.*, 9, 1933, 24-42.

The data do not show, however, that the obtained decrements from interpolated learning may wholly be referred to overt transfer. Somewhat more than a fourth of the decrements may be ascribed to it, but to what shall we attribute the remainder? There are two available, but not equally probable, answers. (1) The first is that the remainder of the inhibition is due to interference effects which do not obtain direct overt expression but which result in a blocking of the correct recall. This is made probable by the results of Experiment II in which the *Ss* reported relatively frequent recalls of the interpolated items during attempted recall of the original list. When these were not recognized as foreign to the list being recalled, they were recorded instead of the correct item. Some items were, as we have seen in Experiment I, recorded and then scratched out. It is at least possible that some of those which were recalled and discarded operated to inhibit the recall of the correct word and it is possible, further, that implicit tendencies to recall may have blocked each other without being reportable. The most probable hypothesis is that at least a major part of the inhibition from interpolated learning not ascribable directly to overt transfer is to be attributed to interference of the kind called negative transfer.

(2) The second and less probable answer is that the remainder of the inhibition, if not all of it, is referable to the diminution by the interpolated learning of a perseveration of the organic events which form the substrate of the original list. It might also be hypothesized that the overt transfer found in these experiments is the explicit result of a perseveration of the more recently learned interpolated list, a perseveration diminished or blocked by no further interpolated learning, which brings it about that the words of the interpolated list are recalled during the attempted reproduction of the words of the original. It is, however, difficult to understand how the complex relations between the character of the original and interpolated activities, which other experiments have shown to be important determiners of retroactive inhibition,[9] could be important determiners on a perseveration theory. It is more difficult still to understand how the meaningful relations between the words of the two lists, which have been shown by the reports of the *Ss* to be dominant conditions of the interference, could play the part they do if perseveration is the major factor. In order to make the perseveration theory fit the facts it would be necessary to endow perseveration with the properties of the materials.

It is simpler and more direct to interpret the facts in terms of the inter-

[9] See, for example, references 1130 to 1145 in J. A. McGeoch, The psychology of human learning: A bibliography, *Psychol. Bull.*, 30, 1933, 1-62, and Learning and retention of verbal materials, *ibid.*, 31, 1934, 381-407.

relations of the materials, as is done in the transfer theory. Before we can assert, however, that transfer is the major determiner of all retroactive inhibition, either of all that found in this experiment or of all that found in others, many more data must be available.

The other errors in recall are not, as we have already seen, a function of condition. The interpolated learning has not produced a greater number of incorrect recalls aside from those resulting from transfer. The fact of the regular decrease in transpositions from Recall 1 to Recall 3 under the work conditions is attributable to the fact that recall was less under work and the opportunities for transpositions were, therefore, fewer. The relatively large numbers of inclusions, although they do not vary with condition, are of interest. They imply that some factors are operating beside those which produce omissions and overt transfers, or at least factors which act in other ways than these two. If we assume that the decrements under rest, and those under work which are not a result of the interpolated learning, are a function of the informally interpolated experiences and activities of the interval, we may hypothesize that the interference produced is of the transfer order and operates to produce confusion between the words in the lists learned and other related words known to the S but not in the lists. The data offer no direct support for this, however, and we shall refrain from debating the merits of the hypothesis. We may conclude that the formally interpolated learning operates to produce omissions, some of which are a function of direct transfer from list to list, but that it exercises no regular influence on errors other than those of overt transfer of the items of one list to those of another.

Summary

In two experiments the delayed recalls of adjectives learned by the method of complete presentation, and followed either by no formal learning or by the interpolated learning of a list of synonyms, have been examined in a search for direct evidence of transfer of items from one list to another.

(1) The two experiments agree in finding considerable amounts of overt transfer from one list to another under the work conditions. The amount of transfer from the interpolated list to the original is much larger than that from the original list to the interpolated. Enough occurs in the latter direction, however, to imply that retroactive inhibition is a two-way interference phenomenon. The overt transfer in both directions accounts, by a conservative estimate, for at least a quarter of the obtained inhibition. The transferred items take the place of the correct items and, thus, decrease the amount of the correct recall.

(2) The reports of the *S*s and the presence of cases of transfer which had been written down as correct and then scratched out show that the actual interference effects resulting from transfer are only partially represented by the cases of overt transfer.

(3) The facts imply that the temporal locus of the inhibition is at the time of recall.

(4) The interpolated learning does not exercise any regular influence on other errors, viz., substitutions, inclusions and transpositions; nor does it affect the appearance of reminiscent items.

(5) The results constitute direct evidence for transfer as one major condition of retroactive inhibition. The most probable interpretation is that transfer is the chief determiner of at least a much larger percentage of the inhibition than can be accounted for by overt transfer alone.

ALLEGED EFFECTS OF FIGURE-GROUND UPON HUE AND BRILLIANCE

By G. A. FRY[1] and V. M. ROBERTSON,[2] Washington University

In the figure-ground phenomenon part of a stimulus-pattern is seen as organized figure and the rest as amorphous 'ground.' Certain investigators have alleged that perceiving a surface as 'figure,' as compared with perceiving it as 'ground,' affects its hue and brilliance. Other investigators have reported instances in which the color of a certain area seems to depend upon its colligation with a neighboring area to form a figure. These effects have been regarded as direct, *i.e.* it has been supposed that the central processes subserving the mode of perception directly affect the cortical processes initiated by the stream of impulses coming in from the eye in such a way as to change the hue and brilliance of the resulting quality.

We have investigated certain of these phenomena and have found in some cases that the observed effects instead of being due to the mode of perception are due rather to neglected factors in the conditions of ocular stimulation. In other cases, changes in brilliance or hue are associated with perceptive reorganization of the stimulus-pattern, but these effects are brought about *indirectly* by motor adjustment of the eye. In no case investigated have we found clear evidence of any *direct* effect.

Retinal interaction. With a stimulus-pattern similar to Fig. 1, Benary has demonstrated that if the two triangles are photometrically equal, triangle B appears more brilliant than triangle A.[3] The parts of the triangle bounded by black and white are identical in the two cases, but white is more predominant in the region of triangle B than triangle A, and, according to the current theories of simultaneous contrast, the latter should be, if anything, the more brilliant, but the reverse is the case.

Ordinarily the cross-shaped black area including triangle B is seen as figure and the surrounding white area, including triangle A, is seen as ground. Benary alleged that the perceptual inclusion of triangle B as part of the figure helps to account for the difference in brilliance between the two triangles.

* Accepted for publication March 16, 1934.

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[3] W. Benary, Beobachtungen zu einen Experiment über Helligkeitskontrast, *Psychol. Forsch.*, 5, 1924, 131-142.

FIG. 1

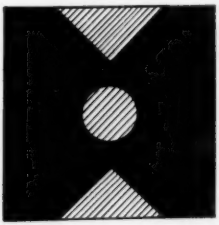

FIG. 2

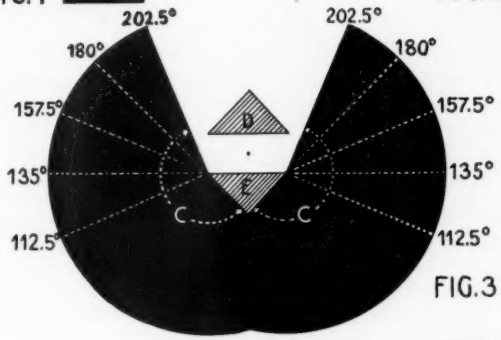

FIG. 3

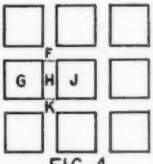

FIG. 4

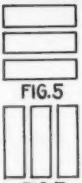

FIG. 5

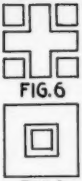

FIG. 6

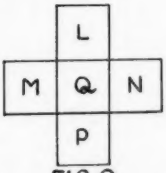

FIG. 9

FIG. 7

FIG. 8

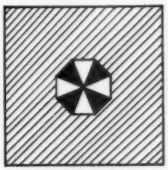

FIG. 10

FIG. 11

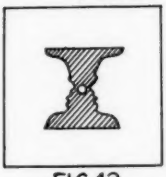

FIG. 12

FIGS. 1-12. STIMULUS-PATTERNS USED IN THE VARIOUS SECTIONS OF THE STUDY

Benary definitely rejected the possibility that differences in the stimulus-conditions of the two triangles might affect physiological processes at a peripheral level in such a way as to produce the effects actually obtained. Mikesell and Bentley[4] considered the possibility whether "mutual excitatory influences of neighboring retinal areas" could account for the facts, but they were unable to place their fingers upon the crucial stimulus-factor affecting interaction.

There is one difference in the stimulus-conditions of the two triangles, not specifically dealt with by previous investigators, but which might affect interaction in a special way. Each of the two triangles is joined by a black and a white field. The two legs face the black field in each case and the hypotenuse the white field, but the lines dividing the white field from the black are continuous with the legs of triangle A and with the hypotenuse of triangle B.[5] In order to show that this stimulus-factor affects brilliance, and that this effect is adequate to account for Benary's phenomenon, the following experiment was performed.

A series of patterns was designed (see Fig. 3) in which angle C varied from 112.5° to 202.5°. The legs of the right triangles were 3/8 in. long, the gap between the two triangles was 1/4 in. wide, and the radius of the black sectors 1 in. long. The distance from the eye was 12 in. A point midway between the two triangles was fixated. The pattern was not observed as illustrated but turned sidewise so that the triangles were seen side by side, D to the right of E, rather than one above the other. The brightness of the field was set at 1.68 c. per sq. ft. and that of triangle E at 0.79 c. per sq. ft. That of triangle D was gradually adjusted until the two triangles were seen equally brilliant. The results are graphically presented in Fig. 13. The value in each case is an average of ten readings. The brightness of D is a relative measure of the brilliance of E. The fact that triangle E is seen more brilliant when angle C is 135° than when 180° accords with the finding of Benary that triangle B in Fig. 1 is seen more brilliant than triangle A.

The possibility of attributing the differences in brilliance to differences in perceptual relationships rather than to changes produced in the peripheral physiological processes was ruled out because perceptual relationships were held constant. The two triangles were seen as windows, and the white strip separating them as a bridge across the center of what might have been a single diamond-shaped opening. The black area was seen simply as a part of the screen containing the two triangular openings. Therefore triangle E was included neither as part of the black nor as part of the white and this perceptual attitude was voluntarily maintained for all sizes of angle C. The one of the authors who served as *O* in the experiment re-

[4] W. H. Mikesell and M. Bentley, Configuration and brightness contrast, *J. Exper. Psychol.*, 13, 1930, 1-23. Compare also J. G. Jenkins, Perceptual determinants in plane designs, *ibid.*, 13, 1930, 24-46.

[5] If retinal interaction can be affected by this factor in a special way, the nature of the interaction must be conceived to be more complicated than is prescribed by the theories of Hering and McDougall, which were advanced to explain ordinary contrast phenomena.

ported no difficulty in maintaining this same attitude for every observation, *i.e.* he was able to maintain the attitude while equating the brilliances of the two triangles.

In order to prove that, with the stimulus-conditions held constant, mere inclusion or exclusion of a triangle as part of a figure does not change its brilliance at all, the following experiment was performed.

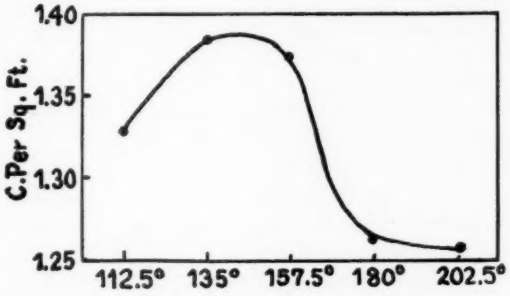

FIG. 13. CURVE SHOWING THE RELATION BETWEEN THE BRIGHTNESS OF TRIANGLE D (IN FIG. 3) AND THE SIZE OF ANGLE C

The triangles in Fig. 2 may be united to the black to form a square, or they may be excluded from the black area which is then seen as the side view of a spool or pulley. The change from one mode of perception to the other is easily accomplished by *imagining* for a moment the opposite configuration from the one *perceived*, and either one may be held long enough to equate the brilliance of the disk with that of the upper triangle. The success of the experiment depends upon this ability. The right eye of *O* was stimulated through an artificial pupil 2 mm. in diam. at a point

TABLE I

BRILLIANCE UNDER INCLUSION AND EXCLUSION

	Triangles included		Triangles excluded	
Obs.	Brightness of the disk	A.D.	Brightness of the disk	A.D.
F	.573 c. per sq. ft.	.013	.570 c. per sq. ft.	.010
R	.627 c. per sq. ft.	.020	.620 c. per sq. ft.	.020

midway between the disk and the upper triangle. The pattern was 12 in. away. The sides of the square were 1½ in. long, the hypotenuse of the right triangles ¾ in., and the diameter of the disk ⅜ in. The brightness of the triangles was set at 0.88 c. per sq. ft., that of the white field at 2.55 c. per sq. ft., and that of the black area at 0.15 c. per sq. ft. That of the disk was adjusted gradually by *O* until the disk was seen as equally brilliant with the upper triangle. The results are given in Table I.

The brightness values represent averages of 10 readings. Measurements were made alternately with the triangles included and excluded. The disk was seen in both cases as a part of the figure. No difference in brilliance occurs.

The above experiment merely supplements and substantiates some experiments by Mikesell and Bentley which bear upon the same problem. In one experiment, for example, they investigated whether a gray horizontal bar adjoined to the base of a vertical black bar was seen more brilliant when it was perceptually colligated with the vertical bar to form an L than when it was seen as a separate object. The *O*s compared the brilliance of the gray to that of another gray of the same photometric intensity in a different part of the field of vision. The percentage of times the one was seen more brilliant than the other was taken as a measure of the difference in brilliance. This percentage was practically the same with and without colligation. Such results, as the authors point out, "offer slight support for the alleged lightening under configuration."[6]

Filling in of gaps. The most striking example of the filling in of gaps in an after-image has been described by McDougall and can be demonstrated with a pattern like Fig. 4, which consists of nine white squares on a black ground.[7] When a point at the center is fixated for some time, and the eyes are then closed and shaded, the negative after-image will so fill in as to give the appearance at one time of a group of three horizontal bars (Fig. 5) and at another time of a group of three vertical bars (Fig. 7).

McDougall regarded himself as able to change from horizontal to vertical, or *vice versa,* by *imagining* the opposite pattern from the one *perceived.* But an *O* cannot be so sure of this ability as he is of moving his arm, because after he decides to change the after-image from one pattern to the other, a considerable time interval elapses before the change actually comes and *O* is left with the doubt whether it was expedited in any degree by his preperceiving or whether it came simply of its own accord. *O* may think himself able to shorten the interval preceding the change by comparing it with the interval required when he assumes a passive attitude, but his judgment would certainly have more weight if it were supplemented by an actual timing of the interval in the two cases.

But even though we might believe that the shortening of the interval can be demonstrated as a fact, we do not have to grant that the change in the manner of filling in is brought about *directly* by the preperceiving unless it can be shown that motor adjustments of the eye are not responsible. One cannot rely upon introspection to detect motor adjustments, for these may occur without the *O*'s knowledge, and their effects have to be demonstrated in an indirect way, for example, by comparing the effects obtained with the intrinsic muscles of the normal eye with the effects obtained when the muscles are paralyzed. Further investigation along this line is necessary before any definite conclusion can be reached.

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[6] Mikesell and Bentley, *op. cit.,* 15-17.

[7] W. McDougall, Physiological factors of the attention process, *Mind,* 15, 1906, 338-339.

If preperceiving were a very potent factor in determining what particular gaps fill in, one might obtain patterns like Fig. 6 and Fig. 8; but this, so far as we have been able to observe, is impossible.

Rothschild presented three patterns,[8] all very similar, which illustrate the filling in of gaps in an after-image. He asserted that the stimulus-pattern, in spite of its gaps, suggests a complete figure and that this perception in turn brings about the filling in but he offered no proof whatever of this proposition. He used only patterns which suggest definite complete figures. He presented no case to show that failure to fill in can be attributed to failure of the pattern to suggest a complete figure. He presented no ambiguous patterns like McDougall's to show that changing the mode of perception causes different gaps to fill in. Nor did he explain why retinal processes cannot account for the filling in.

The same general criticism can be applied to Rothschild's second proposition that *slight* irregularities in a straight or curved line are obliterated in an after-image because these irregularities are incompatible with the *perceived* smoothness of the line.[9]

Color assimilation. The pattern in Fig. 9 is similar to one used by Fuchs for demonstrating the effect of mode of perception upon color assimilation.[10] Two green squares (M and N) and two orange squares (L and P), each ½ in. by ½ in., were pasted upon a black card. Square Q was cut out so that a white surface could be seen through it. After one observes the pattern steadily for about 30 sec., the center square will assume, say, an orange color and this in turn gives place to green, and the two colors continue to alternate. At times the center square is neither green nor orange but white. If it changes from green to white it may change back to green instead of changing to orange. The center square may assume a green color even though a brightness contrast-border separates it from the green squares on either side, but at times the borders disappear and the rectangle becomes uniform.

When the center square, Q, is seen as white, the pattern is seen as a group of five separate squares. When it turns green, M and N are perceptually colligated with it, and the three form a unitary green rectangle. When Q turn orange, LQP forms a unitary orange rectangle. Nevertheless the mere concomitance of the two phenomena, (1) the uniformity of the color of a rectangle resulting from assimilation, and (2) the perception

[8] H. Rothschild, Über den Einfluss der Gestalt auf das negative Nachbild ruhender visuelle Figuren, *Arch. f. Ophthal.*, 112, 1923, 9-10.

[9] *Ibid.*, 2-3.

[10] W. Fuchs, Experimentelle Untersuchungen über die Änderung von Farben unter dem Einfluss von Gestalten, *Zsch. f. Psychol.*, 92, 1923, 251.

of the rectangle as a unitary figure, does not prove which one is the cause of the other. It is possible to conceive either (a) that color assimilation is antecedent to figure perception, or (b) that changes in color assimilation are subsequent to fluctuations in figure perception.

There is, however, another approach to the problem. When MQN is seen as a green rectangle, the change of Q from green to orange may be delayed by concentrating one's attention upon MQN or it may be hastened by imagining LQP to be a unitary orange rectangle. The change from orange to green can be manipulated in a similar way. The voluntary control is not perfect, however. The color may change to white instead of orange or green as desired, or it may change from white to orange while one is trying to make it change to green or *vice versa*. This voluntary control of assimilation is, as the following experiment shows, brought about

TABLE II

COLOR CHANGES THROUGH MOTOR READJUSTMENTS

Squares	Color	Brightness
M and N	Green	.79 c. per sq. ft.
L and P	Orange	.69 c. per sq. ft.
Q	White	1.15 c. per sq. ft.

indirectly by motor adjustments of the eye. Although other explanations are conceivable, the simplest explanation of this fact is to assume that color assimilation involves retinal processes which are directly affected by motor adjustments of the eye, and, if this is true, changes in color assimilation must be conceived to be antecedent to changes in figure perception.

The brightnesses of the squares as measured with a Macbeth illuminometer are given in Table II.

The right eye was blinded and the left eye looked through an artificial pupil 2 mm. in diam. at a fixation-point at the center of Q. The distance of the eye from the card was 12 in. We measured the total number of red, green, and white phases of Q during the minute beginning 30 sec. after the onset of stimulation, both when attempts were made to accelerate the rate of alternation and when they were made to retard it. Observations were made with both eyes paralyzed by homatropine and with both eyes normal. The results are given in Table III. The values represent averages of two trials for each of the two *O*s.

The results indicate that the voluntary control of the rate of alternation is exercised through the intrinsic muscles of the eye, which can bring about three changes, viz., (1) in the sharpness of the optical image upon the retina, (2) in the intraocular pressure, and (3) in the size of the pupil. Changes in the size of the pupil were made ineffective by the use of an artificial pupil. The effect upon color assimilation must then be attrib-

uted to changes in the intraocular pressure or in the sharpness of the retinal image. How either of these changes can affect color assimilation we have not determined, nor are we able to account for the connection between these motor adjustments and processes in the brain which initiate them.

Size of pupil. It has been demonstrated by Hartmann in observing the Rubin cross that the rate of succession of two short exposures necessary to get fusion is higher when the white cross is seen as figure than when seen as ground.[11] In explaining this effect, Koffka assumed that the seat

TABLE III

COLOR PHASES UNDER PARALYSIS

O	Eyes normal				Eyes paralyzed			
	Retardation		Acceleration		Retardation		Acceleration	
	Rate per Min.	A.D.	Rate per Min.	A.D.	Rate per Min.	A.D.	Rate per Min.	A.D.
R	8.54	.035	19.65	.35	12.55	.79	13.88	1.12
F	15.04	2.85	24.72	1.1	17.29	1.34	15.3	2.5

of fusion is in the cortex.[12] He postulated further that seeing the white as figure, as compared with seeing it as ground, has a direct effect upon the stream of impulses coming in from the retina the same as if the brightness of the stimulus had been increased.

Results which we have obtained with an artificial pupil throw a different light upon this phenomenon.

We used the pattern in Fig. 10. The black sectors were pasted upon a gray card and the white sectors were cut out and intermittently illuminated from the back. The off-and-on phases of the intermittence were equal. The diameter of the pattern of sectors was 1½ in., that of the field 4 in. The pattern in an otherwise dark field was observed from a distance of 12 in., the center of the pattern being the point of fixation. The brightness of the gray field was 0.36 c. per sq. ft., of the white sectors 1 c. per sq. ft. The pattern was observed monocularly both with the natural pupil and with an artificial pupil (2 mm. in diam.). The results are given in Table IV. The values represent averages of 5 readings.

The results show that, with the natural pupil, the critical flicker frequency is higher when the white is seen as figure than when seen as ground; but with an artificial pupil this difference disappears. A possible

[11] L. Hartmann, Neue Verschmelzungsprobleme, *Psychol. Forsch.*, 3, 1923, 319-396.

[12] K. Koffka, Perception: An introduction to the Gestalt-theorie, *Psychol. Bull.*, 19, 1922, 565-566.

explanation is that when one attempts to see the white as figure, the pupil dilates, allows more light to enter the eye, and raises the critical flicker frequency. We are not prepared to explain how the pupil comes to change its size or what function this change serves. But one thing is certain, that though an artificial pupil is used, or even though the iris and

TABLE IV

CRITICAL FLICKER FREQUENCY WITH NATURAL AND ARTIFICIAL PUPIL

O	Natural pupil				Artificial pupil			
	Black as figure		White as figure		Black as figure		White as figure	
	Critical flicker frequency	A.D.	Critical flicker frequency	A.D.	Critical flicker frequency	A.D.	Critical flicker frequency	A.D.
R	39.58	0.14	42.06	.87	29.74	.42	29.86	1.26
F	39.80	1.04	43.02	.90	35.28	.90	35.74	0.97

lens muscles are paralyzed, either the white or the black may be seen as figure.

In order to substantiate the explanation of the change in critical flicker frequency given above, the following experiment was performed to demonstrate that the size of the pupil does change under the different modes of perceiving Fig. 10.

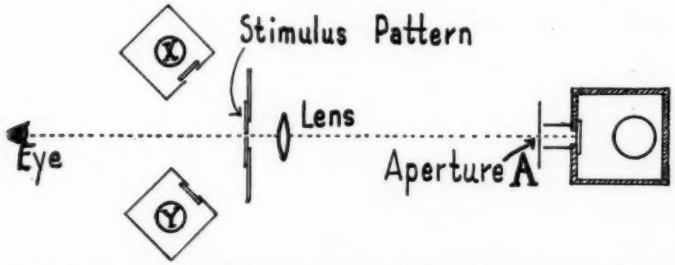

FIG. 14. APPARATUS FOR MEASURING THE SIZE OF THE PUPIL UNDER VARIOUS MODES OF PERCEIVING A REVERSIBLE FIGURE-GROUND PATTERN

The apparatus used in the experiment is illustrated in Fig. 14. The stimulus-pattern was like Fig. 10 and was illuminated by light sources X and Y. The brightness of the black sectors was 0.05 c. per sq. ft., that of the white sectors 0.305 c. per sq. ft., and that of the gray field outside the pattern 0.115 c. per sq. ft. A pinhole at the center of the stimulus-pattern allowed a small beam of light to pass through so that the lens could focus an image of Aperture A (0.013 in. in diam.) upon the pupil. Aperture A could be displaced horizontally and vertically and its position in both directions could be determined to within 0.0003 in. *O*'s head was

held steady by a biting board. When the image was moved across the pupil it was easy for *O* to tell when it reached the edge of the pupil because the pinhole suddenly became dark. First of all the apparatus was adjusted so that lateral displacement of the aperture caused the image to move across the horizontal diameter of the pupil. An attempt was then made to measure the positions of the aperture when it fell on opposite edges of the pupil under the two perceptual conditions; namely, with the white cross seen as figure and then the black cross. Six pairs of alternate readings were made for the two modes of perceiving the pattern for the left edge of the pupil, and similar readings were made for the right edge of the pupil. *O* adjusted the position of the aperture himself. The diameter of the pupil when the white cross was seen as figure was calculated in the case of observer *F* to be 0.222 in., and 0.212 in. when the black cross was seen as figure. The average errors for the four measurements ranged from 0.0012 to 0.0016 in. This experiment substantiates the fact that the pupil is larger when the white cross is seen as figure.

The alleged dependence of threshold. Gelb and Granit have reported an experiment which seemed to them to indicate the dependence of threshold upon the figure-ground mode of perception.[13] They used a dark cross upon a bright field (Fig. 11) and another pattern in which this brightness relationship was reversed. A red dot could be seen as part of the pattern in either of the two positions indicated, but it was admixed to the pattern by being reflected by a cover-glass placed at an angle in front of the pattern. In each case the dot served as the point of fixation. The threshold brightness of the red dot was determined for each of its two positions (on figure and on ground) in each of the two patterns. The order of readings and a typical example of the results are given in Table V.

TABLE V

BRIGHTNESS THRESHOLD AND FIGURE

(Experiment of Gelb and Granit)

Order of readings	Position of the red dot	Brightness threshold (Relative Brightness)
1	Dark Figure	2.48
2	Dark Ground	1.68
3	Bright Ground	9.35
4	Bright Figure	10.57

From 4 (Bright figure) *O* returned to 1 (Dark figure) and repeated until 5 readings of each were made. The values indicate in each case the average for the 5 readings. Since the red dot has a higher threshold on figure than on ground in the case of both the dark and the bright, Gelb and Granit generalized that the threshold is higher on the figure because it is figure, but they overlooked certain factors in the stimulus-conditions to which the results may be attributed.

One such factor is the order of readings. They chose an order in which the thresholds for both dark and bright figures were measured immediately after the

[13] A. Gelb and R. Granit, Die Bedeutung von "Figur" und "Grund" für die Farbenschwelle, *Zsch. f. Psychol.*, 93, 1923, 83-118.

critical part of the retina had just previously been fatigued by exposure to a bright stimulus, and the thresholds for ground were determined after the retina had been relatively rested by exposure to only a weak stimulus. From the known facts about fatigue one can expect to get a higher threshold with a fatigued retina than with one which is relatively rested.

The substitution of the one pattern for the other is undesirable because it changes (1) the size of the pupil, (2) the amount of light diffused throughout the interior of the eye, and (3) the conditions affecting interaction of the different parts of the retina, and each of these factors affects the threshold.

We have avoided the difficulties inherent in Gelb and Granit's method by using a pattern like Fig. 12 whose parts can be seen interchangeably as figure and ground, without involving any change in the stimulus-conditions, and in such a case we have found that there is no significant difference between the threshold on figure and on ground.

The distance from the pattern to the eye was 12 in. Observation was monocular. An artificial pupil 2 mm. in diameter was used. The brightness of the goblet was 0.9 c. per sq. ft., of the faces 1.95 c. per sq. ft. Readings for figure and ground were taken alternately, and a 15-sec. rest period followed each observation. The averages for 10 readings are given in Table VI. The values represent voltages of the lamp illuminating the red dot when it becomes just visible.

TABLE VI

BRIGHTNESS OF REVERSIBLE GOBLET

O	Goblet figure	A.D.	Goblet ground	A.D.
R	64.40	1.60	63.45	2.05
F	71.30	2.39	70.75	2.65

Summary and conclusions. We have investigated a number of the phenomena proposed by previous investigators as demonstrating that the figure-ground mode of perception directly affects hue and brilliance. According to our results none of the phenomena which we have investigated furnish any clear evidence of the alleged relation, inasmuch as the effects presented as evidence of this relation may be attributed in some cases to motor adjustments of the eye and in other cases to neglected factors in the stimulus. In certain cases of color assimilation and filling in of after-images, we have found subjective changes in color which were not correlated with changes in the stimulus-conditions because these were kept constant, but they were correlated with changes in perceptual configuration. These changes in color, however, have not been proved to result from the changes in configuration.

After showing in a few cases that the figure-ground mode of perception does not affect hue and brilliance, we cannot, of course, generalize that this

never occurs, because we have not shown why it cannot happen. But the theoretical and practical importance of the solution of this problem cannot be denied, for if the figure-ground mode of perception cannot affect color except *indirectly* by motor adjustments of the eye, and if these indirect effects can be prevented by the use of devices like artificial pupils, fixation-points and mydriatics, psychologists interested in the investigation of sensory phenomena under controlled conditions do not need to fear the advent into the experimental situation of the subjective figure-ground factor which has been alleged to play havoc with hue and brilliance. Futile are the attempts of psychologists to elucidate the processes underlying the figure-ground mode of perception by saying that they must be of such-and-such a nature in order to produce such-and-such effects upon hue and brilliance.

THE SIZE-CONSTANCY PHENOMENON IN STEREOSCOPIC SPACE

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The principle of phenomenal constancy is a major principle of the psychology of perception, one which no adequate theory of perception will venture to ignore. Since Hering's attention to the problem, especially as it appears in vision, a rich literature on the subject has appeared in Germany; but English and American experimenters have generally (except G. K. Adams, R. H. Thouless, R. B. Macleod, K. Koffka, and a few others) overlooked this interesting and significant problem. The constancy phenomenon refers to the tendency of the observer to perceive objects and their specific aspects with relative constancy despite changes in the conditions of stimulation. Thus colors and brightnesses are perceived with relative constancy in spite of changes in the quality and the energy of illumination. A white paper seen in deep shadow appears brighter in direct vision than it does on the film of a camera. Nor is vision 'photographic' as regards the apprehension of the size and shape of objects. Moreover, the principle of constancy may be as marked in hearing, in touch, and in kinaesthetic perception as it is in vision. Except in vision however, the field is practically unexplored.[1]

The constancy tendency immediately strikes the functional and the 'purposive' psychologist as a device by means of which the creature restores the 'real' characteristics of an object despite adverse stimulus-conditions. Thouless speaks of the process as "phenomenal regression to the 'real' ob-

* Accepted for publication April 11, 1934.

[1] The reader will find an excellent introduction to the experimental and theoretical work of the German authors in R. B. Macleod, An experimental investigation of brightness constancy, *Arch. Psychol.*, 21, 1932, (no. 135), 1-101. This study also reports a long series of experiments on brightness constancy. R. H. Thouless reports a series of experiments on the constancy of size, shape, and brightness, and shows the bearing of constancy effects on the phenomena of simultaneous and successive contrast, the size-weight illusion, and on Weber's and Merkel's formulations of the psychophysical law (cf. Phenomenal regression to the 'real' object, *Brit. Psychol.*, 21, 1931, 339-359; 22, 1931, 1-30; Individual differences in phenomenal regression, *ibid.*, 22, 1932, 216-241. G. K. Adams extends the experiments into every sense department (cf. An experimental study of memory color and related phenomena, this Journal, 34, 1923, 359-407). K. Koffka shows experimentally that constancy harmonizes with certain principles of *Gestalt* (cf. A new theory of brightness constancy, a contribution to a general theory of vision, *Stud. Physic. Soc.*, 1932, 182-188; also Some remarks on the theory of colour constancy, *Psychol. Forsch.*, 16, 1932, 329-354. A brief account of theories and a statement of the results of an experiment on brightness constancy during changing illumination is reported by C. O. Weber (*J. Exper. Psychol.*, 16, 1933, 815-830).

ject." Some experiments of Gelb's, however, cast doubt upon the Darwinian interpretation by describing experimental conditions in which the constancy process causes an object to vary away from its real or ordinary characteristics. In any case, it was soon noted that this correction is seldom complete. A white paper seen in twilight appears lighter in vision than it does in photography. The camera adds to the paper the darkness of surrounding illumination, which the eye, so to say, sets aside. The brightness seen, however, is intermediate between the albedo of the paper in daylight and the albedo given by the camera. An inclined circle is seen as more circular than the form which the laws of projection give, but not all of the circularity is restored. Thouless clarifies the matter by noting that we are concerned with three objects: (1) the 'real' object (*e.g.* the circle), (2) the stimulus-object (the ellipse which the inclined circle would give in photography), and (3) the phenomenal object, the form as perceived by the observer. The phenomenal object always lies midway between the real and the stimulus object, being a 'compromise' between them. Thouless suggests a serviceable formula for expressing the degrees of regression to the real object which enables us to compare the amounts of regression in widely diverse studies of it.

The present study is concerned with the constancy of the perceived size of objects which vary in distance from the observer.

The problem attracted attention long before the Hering-Helmholtz controversy regarding 'memory colors.' Malebranche in his *Recherche*[2] wrote as follows:

"A dwarf, two paces from us, certainly appears much smaller than a giant, three times as large, who is distant six paces, although both are seen under equal angles, or what is the same thing, although the images which are traced on the fundus of the eye are equal. As this inequality in the apparent magnitude does not arise from the inequality of the visual angles, or of the images which certainly are equal in the fundi of our eyes, it must be due to the inequality of the distance."

Malebranche not only observes the phenomenon but makes an astute suggestion regarding the mechanism of the correction. The criteria which enable us to make the correction arise, not from remembered knowledge, but from *present cues of perception*. Thus, we know that the sun is many times the size of the moon, but we see them as having about the same size. This is because *nothing else occurs in vision* which signifies their varying distances. The intervening space between these stellar objects and the observer is not seen because it is in fact empty. In the case of the dwarf and the giant, intervening space is made apparent by intervening objects and the ground. Recent experiments unite in showing that the constancy-correction is not learned in the ordinary sense. Children and animals may make corrections as large as, or larger than, adults. Like ordinary illusions, the constancy effect persists despite knowledge. Thouless has shown in a crucial experiment, however, that the

[2] See N. K. Smith, Malebranche's theory of the perception of distance and magnitude, *Brit. J. Psychol.*, 1, 1904, 191-204.

factors which determine constancy arise from present perception, by proving that an inclined circle and an equivalent ellipse are indistinguishable when all cues to their actual shapes are eliminated.[8]

Assuming that the cues for the size-correction do appear in immediate perception it became the problem of the present study to determine what the factors are which determine regression to real size. The factors which naturally suggest themselves are the traditional binocular cue of discrepant images and the so-called 'psychological' factors of clearness, superposition, height above a base-line and apparent size. The cue of apparent size must not be included in this experiment, for it would be a circular argument to assert that the distance of an object, revealed through its reduced size, is the cue for seeing it larger. To the extent that size is itself the cue for distance perception, distance cannot logically account for regression to real size. The following study, throwing light on several of the factors involved gives especial attention to the rôle played by disparation. The stereoscope was used to vary this factor.

The Experiments

There are three experiments to report, the first two of which were made in 1932 and 1933 and were preliminary to the experiment of 1934.

Preliminary experiments. (a) Materials and procedure. In the 1932 experiment, 8 college women judged the areas of photographed disks of three sizes (20, 25, and 30 cm. in diam.), each disk being photographed at 5, 10, and 15 yd. The photographs were taken on an open lawn bordered on the left and in the background by trees. A boy 9 yr. of age, familiar to the *O*s, held the disk while it was photographed. The camera used (Eastman no. 116) was moved laterally 65 mm. to take the second photograph of each pair forming a stereoscopic slide. The slides, 9 in all, were inserted into the stereoscope (Brewster) and so viewed by the *O*s.

The *O*s were instructed to adopt the phenomenal attitude rather than the object-directed attitude. They were instructed to gauge, not the area of the disk photographed, but the area of the photograph of the disk. It is well known that this phenomenal attitude reduces the constancy-correction, but has the virtue of giving uniformity of conditions and simplifying the task. Had the *O*s been instructed to gauge the areas of the real disks, it would have led to wild guesses regarding real size, regarding which *O* has no cues except the comparative size of the boy and the trees! The phenomenal attitude was used in all the experiments. After gauging phenomenal area, *O* turned to a chart of comparison areas, 25 in number, and designates an equivalent area. Five reports were made for each slide.

In the 1933 experiment this material was used with 10 women *O*s who judged each slide 5 times in the stereoscope, and then 5 times with unaided vision. This procedure was designed to bring out the difference, so far as regression is concerned, between stereoscopic vision, which gives disparation, and normal vision, which in this case would give little disparation since the object observed was a flat disk.

(b) Results. Table I gives the average results of the 8 *O*s from nine slides. Across the top the heading A, B, and C designate the disks whose real diameters were 20, 25, and 30 cm. The suffixes 1, 2, and 3 represent the distances of 5, 10,

[8] R. H. Thouless, *op. cit., Brit. J. Psychol.*, 22, 1931, 6.

and 15 yd. at which each disk was photographed. At the left, the stub, "Av. Pa." means the average phenomenal area (in sq. mm.) selected by the *O*s as equal to each disk. "Av. D." gives (in percentages) the deviations of the area-judgments from each *O*'s average for each circle. These deviations are large for all *O*s throughout all our experiments. We believe that the large deviations are mainly due to a conflict of attitudes. Seeking to gauge the phenomenal areas of the photographs, *O* is beset by an inveterate tendency to see the real disk photographed, so dominating is the impression of reality in the stereoscope. The line "Real S.A." records the real areas of the photographs of the disks as magnified by the lenses (2.035 times the areas of the disks on the photographs). "Phot. R." gives the quotients resulting when phenomenal areas are divided by real stereoscopic areas; *e.g.* the disk on slide A1 is seen 2.8 times its real size, while the same disk on A3 (15 yd.) is 10.9 times its real stereoscopic size.

TABLE I

RESULTS OF THE 1932 EXPERIMENT

	A_1	A_2	A_3	B_1	B_2	B_3	C_1	C_2	C_3
Av. Pa.	152.2	86.9	56.7	253.9	139.7	88.3	372.5	197.8	107.6
Av. D.	16.3	23.4	25.6	14.6	18.8	23.2	4.0	18.8	23.0
Real S. A.	53.8	12.5	5.2	82.9	18.5	9.2	118.2	28.2	12.5
Phot. R.	2.8	6.9	10.9	3.1	7.6	9.6	3.2	7.0	8.6
Dis. R.		57.1	37.3		55.0	34.8		53.0	28.9
Opt. R.		23.3	9.6		22.3	11.1		23.8	10.6

We have here one of the novel results of our first experiment which reappears in all later ones. There appear to be two size-corrections. One is the tendency to see photographed objects as nearer their true area than photographic dimensions warrant. This regression, as usual, is a compromise between real physical size of the object and its stimulus-conditions, as represented by the photographs. In addition to this correction (noted for the first time, we believe), there is the usual correction for distance. It is impossible to say what the phenomenal size of an object would be at zero distance. We shall assume the area seen at the distance of 5 yd. as an arbitrary distance of zero, and then consider what percentage of this area is retained when the same disk is moved to 10 and to 15 yd. The line "Dis. R." gives the results in question. Thus Disk A at distance 2 still retains 57.1% of its size at distance 1, at distance 3 it retains 37.3% of its original size. Line "Opt. R." gives the corresponding percentages of size-retention for the camera. When compared the last two horizontal lines bring out the tendency of visual perception to regress to the "real" object, but the regression is never perfect. Individual results conform closely to group-results, no *O* showing a loss of size with distance as large as that which obtains for the camera. The only discrepancy is that in 9 cases out of 24 possibilities, *O*s gave inversions of regression, *i.e.* the exaggeration of size at distance 3 was given as less than at 2. An experiment of this sort is recommended for elementary laboratory courses, as one is practically certain to get positive results even with untrained and relatively careless *O*s.

The experiment of 1933 differs from the preceding in one respect. Every one of the 10 *O*s was required to judge each slide while in the stereoscope, and again with the naked eyes. Five observations of each type were recorded. The areas

selected in the latter procedure are, of course, much smaller than in the former. Since the stereoscopic lenses magnify the image approximately twice its real size, we should expect the phenomenal areas selected with the unaided eyes to be about one-half the areas selected when the stereoscope is used. But, as a matter of fact, the average *O* now selects an area which is only one-fourth to one-eighth the area chosen when the stereoscope is used. This means, as we believe, that when the stereoscope is discarded, *O* loses most of the regression-tendency which we have called 'photographic regression,' *i.e.* to see photographed things as nearer their true size; although the tendency still remains. The pertinent results are found in Table II.

TABLE II

RESULTS OF THE 1933 EXPERIMENT, SHOWING SIZE-CONSTANCY IN STEREOSCOPIC AND IN DIRECT VISION

	A_1	A_2	A_3	B_1	B_2	B_3	C_1	C_2	C_3
Ster. Pa.	213.6	90.0	66.2	281.2	152.7	101.7	418.4	199.3	128.7
Non-S. Pa.	54.3	14.6	8.0	83.3	28.1	12.8	123.6	39.1	18.9
Real. Pa.	26.4	6.2	2.5	40.7	9.1	4.5	58.1	13.9	6.2
Ster. Reg.		44.1	32.4		58.1	40.1		47.7	31.0
Non-S. reg.		26.3	14.1		33.6	14.3		33.9	14.9
Opt. reg.		23.3	9.6		22.3	11.1		23.8	10.6

At the left, "Ster. Pa." indicates the average phenomenal areas selected with the stereoscope; "Non-S. Pa," the areas selected in direct vision, and "Real. Pa.," the real areas of the disks on the photographs. It will be seen that although the areas selected by the *O*s with the naked eyes (second line) are much smaller than those selected when the stereoscope is used (first line), they are still larger than the real areas of the disks on the slides. The last three lines of the table enable us to compare the retentions of size (in percentages) with increasing distance. "Opt. reg." gives the optical or photographic decrease in size, which may be taken as zero regression. Lines "Ster. reg." and "Non-S. reg." show that the eyes retain more of the area as distances are increased than the laws of optics warrant. Nevertheless, the naked eyes viewing the photographs give a smaller correction than they receive when the stereoscope is employed. The differences may be taken as a rough measure of the rôle played by disparation, if we may suppose that difference in discrepancy of images is greater between stereoscopic and non-stereoscopic vision of photographs than are the differences as regards shading, clearness, height above a base-line and other criteria.

Main experiments. We turn now to a more complete account of the experiments of 1934 in which a new set of 18 slides and 28 *O*s are used.

(a) Materials and procedure. Two disks, one of 20 cm. and the other one of 30 cm. diam. were photographed with the same camera and on the same open lawn used in previous experiments. The disks were fastened to an upright stick 140 cm. long, at a height of 65 cm. from the ground, this being the height of the camera objective during exposure. Each disk was photographed at 4, 8 and 12 m. Three series of paired photographs were taken: (1) in which right and left photographs are duplicates, giving identical images; (2) in which the camera was moved

laterally three inches for the second photograph of each pair; and (3) in which the lateral separation was 100 cm. We thus get 6 sets of slides with three slides in each set, *i.e.* one for each of the three distances. The sets consisted of the following: (A) small disk, without disparation; (B) small disk with 3-in. disparation; (C) small disk, exaggerated disparation; and (D), (E), and (F), corresponding sets for the large disk.

The pairs of disks were so mounted on stereoscopic slides that the centers of the disks on all slides are a fixed distance apart and a constant distance from the base of the card. When one slide is placed in the Brewster stereoscope and adjusted for clear vision, it is found that this adjustment suffices for all other slides. Each *O* was required to find and to maintain such a constant adjustment.

An unfortunate complication arises from the fact that the modern stereoscope magnifies the object unequally, although it is intended to magnify it the same amount for all *O*s. The focal length of our stereoscopes is 19.8 cm. This gives a magnification of 2.035 if we take the distance of 20.5 cm. as the point of clearest vision, experimentally determined. Magnification is changed relatively little by changing the distance between the eyes and the lens or the distance between the lens and the slide. But this constancy of magnification for all *O*s suffers appreciably when there is abnormality of vision, especially in the case of presbyopia (*O*s over 45 yr. of age). To offset these effects we avoided *O*s with marked defects of vision, and only one was above 45 yr. Whatever the variations in magnification may be from *O* to *O*, they are too small to obscure the huge constancy-effects which appear. The old Wheatstone stereoscope might be a simpler instrument to use in such experiments.

In the 1934 experiments we had three groups of observers: (1) 8 children (4 boys and 4 girls), 9-12 yr. of age; (2) a group of 10 college women, 16-22 yr., with an average age of 20; (3) a group of 10 men, made up of 2 young men, 7 professors of mature years, and an engineer of 48 yr. of age. The ages of the men were 21-48 yr., with an average of 32.5. We do not consider that our groups justify any conclusive statement regarding age and sex differences in phenomenal regression. Sex and age differences do appear, but, as is usually found in constancy experiments, other individual differences greatly exceed these.

O was instructed to gauge the phenomenal areas of the photographs of the disks, and then to select a disk of equal area from a set of 30 comparison-circles varying in area from 0.78 to 754.9 sq. mm. The 18 slides are judged in chance order. Five series of 18 reports each are made during which *O* sees the slide only after being placed in the stereoscope. This is followed by an equal number of reports during which *O* himself places the slide in position, thus sees them without lenses before observing them in the stereoscope. This gives 10 reports for each of 18 slides. Every *O* completed all observations in one sitting (in about 45 min.). Preliminary trials were given with a practice slide of the 1932 series.

Results. Chief interest centers in the effects of varying disparation on the two types of regression discovered in preliminary experiments. We prepare the way for this problem by noting several accessory results. First, what were the effects of the two procedures used? Other investigators are practically unanimous that constancy effects do not depend materially on 'knowl-

edge about' the object.[4] White paper seen in shadow is corrected for brightness whether or not it had been seen before. The constancy-phenomenon persists despite ignorance just as ordinary illusions persist despite knowledge. Our present results fully confirm this result. If we divide each average phenomenal area by the corresponding real (stereoscopic) area, we find that the quotients are almost identical for the first and second halves of the experiment. Children, on the average, see the object 6.8 times its real size in the first half of the experiment during which the slides are seen in the stereoscope only, and 6.9 times their real size in the second half, during which the slides are first seen with the unaided eyes. The corresponding figures for women are 6.5 for the first half, 7.0 for the second half; while for men the values are 6.5 for first half and 6.6 for second half. These figures also show that age and sex differences are quite small.

TABLE III

COURSE OF 'AREA' REPORTS WITH PRACTICE

Series	Children	Women	Men
1	226.9	207.6	191.7
5	242.8	228.1	203.9
6	228.9	227.8	193.1
10	241.4	240.4	212.6

The procedure with knowledge had the effect of notably reducing the average deviations for all *O*s. The average deviations for all observers was 35.2% during the first half and 19.6% during the second half. Children showed the largest deviations, women the least, and men were intermediate in this respect. It must be recalled that the group of men was heterogeneous.

Do changes occur in the reports as the experiments proceed? In experiments on color constancy the *O*s are usually instructed to maintain the naïve, object-directed attitude since this favors a constancy-correction. We chose instead to secure phenomenal judgments, gaining in uniformity what might be lost in constancy-effect.

Table III shows the changes of phemonenal area during the course of the experiment. Series 1, 5, 6, and 10 (of the 18 series) were taken as representative. It is clear that if phenomenal-area reports increase, regression to the real object tends to increase also. It appears from the table that, despite the phenomenal attitude, continued practice is accompanied with an increasing constancy-correction. It will be recalled that after Series 5, *O* sees the slide before it enters the stereoscope. This change in procedure causes a sudden change towards more phenomenal reports, but the con-

[4] Cf. Macleod, *op. cit.*, Chap 1 and sect. 6 of Chap. 4.

stancy-correction gradually reasserts itself becoming even larger in trial ten than it was in trial five. In previous research on all types of phenomenal constancy, children and animals were found to possess such large amounts of it that the view became prevalent that the constancy-correction is a native and not an acquired tendency. The results shown in Table III would seem to contradict this view and lend support to an empirical theory.

Thouless[5] shows that practice tends to reduce the effect without destroying it entirely. He tested for shape, size, and brightness constancy. Students and teachers of art show regression to some extent but art students show slightly less of it than regular university students. Comparing artists with control groups Thouless found that not one of the artists was above the median degree. He also found a decrease with age. This does not agree with Beryl's results[6] which indicate a slight improvement in size-constancy between the ages of 2 and 10 yr. Frank[7] criticises Beryl's procedure, and after repeating the experiment with corrections finds that younger children succeed somewhat better in gauging true size altered by distance. Beryl had presented two sizes near together and on a level with the eyes, but differing in distance from the *O*. Frank alleges that under these conditions objects tend to be drawn to the same plane. Depth differences tend to be lost under these conditions and the child, less sensitive than the adult to depth-cues, is at a disadvantage. Frank therefore repeats Beryl's experiment, first according to Beryl's procedure and then according to his own, in which the two objects stand apart and are below the level of vision. In the latter procedure, children 5-7 yr. of age make more failures in selecting the physically larger or smaller box than do children from 11 mon. to 2 yr. of age.

Thouless also found that intelligence, as well as age, tends to reduce the effect. The more intelligent *Ss* tend to see the object more nearly in its stimulus-character. He also found that women tend to see slightly nearer the real character of the object, and that men are influenced more by the stimulus-character. These findings regarding sex and age prompted us to bring our data (cf. Table IV) to bear on these problems.

Table IV gives in the extreme left column the designations for the six series of slides used with three slides in each series representing the three distances. Series A, B, and C are of the small disk (20 cm.) and D, E, and F of the large disk (30 cm.). Series A and D are the slides without disparation, B and E show the nearly normal disparation of 3 in. C and F show exaggerated disparation. Column 3 of the table gives the real areas in sq. mm. of the photographs of the disks as magnified by the lenses. The next column (O. reg) gives the optical regression of the slides. The real area at distance 1 (4 m.) is taken as the point of departure. The area of

[5] R. H. Thouless, *op. cit.*, 21, 1931, 234 ff.

[6] F. Beryl, Über die Grössenauffassung bei Kindern, *Zsch. f. Psychol.*, 100, 1926, 344-371.

[7] H. Frank, Untersuchung über Sehgrössenkonstanz bei Kindern, *Psychol. Forsch.*, 7, 1925, 137-146; Die Sehgrössenkonstanz bei Kindern, *ibid.*, 10, 1927, 102-106.

Disk A at distance 2 (8 m.) was 14.4 sq. mm. which is but 25.% of its area in position 1, and at distance 3 (12 m.) it is only 11.1% of its original size. This is the reduction with distance as the camera yields it. Columns 3 and 4 of the table serve as the basis for comparing the results of children, women and men given in the remainder of the table. For each of these groups, the average percentages of deviation are given, which are the largest for children and the smallest for women.

The columns headed "P. reg." refer to what we have called 'photographic regression,' *i.e.* the tendency to see the photographed objects as nearer their true sizes. The figures in these columns are secured by dividing the average phenomenal area given to each disk by the *O*s by the real area of the disk as given in Column 1. Children show the largest regression of this sort (average = 6.66), men show the least of it (6.27), and women are intermediate (6.52). This agrees with the findings of Frank and Thouless already cited that the phenomenon decreases slightly with age, and with the finding of Thouless that women see nearer the real object than do men. The differences are small, however. One must recall also that according to the results shown in Table III, children, women and men alike show an increase during the course of the experiment. If the phenomenon improves during the course of a limited experience, one would expect it to increase also between youth and maturity. Perhaps the improvement shown by Table III is rather a process of adaptation to the stereoscope. It would then suggest the case of an expert typist whose rate of performance is cut down when he changes suddenly to a new machine. In time his performance on the new machine would improve, but this would constitute no advance of his original capacity, but only the recovery from loss.

The column "D. reg." refers to the reduction in size due to increasing distance. The values in this column were computed (as in the case of column 3) by dividing the phenomenal areas given for a certain disk in positions 1 and 2 by the phenomenal area for distance 1. These columns show, as did Tables I and II, that all types of *O* show less loss of size with increasing distance than does the camera. In other words, all Os show regression. This type of regression, however, is greatest for men (av. = 31.67%) least for women (av. = 27.59%).

Individual differences for both types of regression are overwhelmingly greater than any differences due to sex or age. This fact has been verified again and again since the classical studies of Katz.[8] Thouless' study of individual differences is the most recent confirmation of this fact. Thouless took a distinct step in advance in showing by means of partial correlations that the individual who shows large

[8] D. Katz, *Der Aufbau der Farbwelt*, 1930, 215-223.

phenomenal regression in one test tends to show it large in other tests also. Indeed, one may speak of a group factor of regression. Our own results amply confirm the findings regarding the wide extent of individual differences. Among the men, for instance, one *O* (a psychologist) saw the disks quite near their true size, giving Disk A1 an area of 80, while another *O* (an engineer) assigned an area of 300 sq. mm. to this same disk. The true area was 57.5 sq. mm. The differences between these two cases are very likely the differences made by the psychologist's inveterate 'phenomenal' attitude, and the object-directed attitude of the man of affairs. Katz,

TABLE IV

RESULTS OF THE EXPERIMENT

Series	Distance	OBJECTS		CHILDREN			WOMEN			MEN		
		Areas	O.reg.	pc.d.	P. reg.	D. reg.	pc.d.	P. reg.	D. reg.	pc. d.	P. reg.	D. reg.
A	1	57.5		14.7	6.0		6.8	5.7		9.3	5.1	
	2	14.4	25.0	20.9	8.2	34.2	9.8	7.7	34.0	15.1	7.5	37.0
	3	6.4	11.1	16.9	9.9	18.3	11.6	9.2	17.9	15.5	10.5	23.1
B	1	57.5		13.8	5.2		8.6	5.7		9.9	4.6	
	2	14.4	25.0	20.4	7.7	37.2	13.8	7.2	31.5	14.0	7.3	39.3
	3	6.4	11.1	11.5	10.6	22.8	9.9	9.4	18.5	10.1	9.1	21.8
C	1	57.5		15.3	4.1		8.4	4.4		11.9	3.9	
	2	14.4	25.0	14.8	6.8	41.8	13.7	7.1	40.9	15.9	6.8	43.2
	3	6.4	11.1	12.1	8.5	23.4	10.2	8.2	21.0	10.9	8.8	24.9
D	1	129.5		6.9	5.0		4.9	4.8		7.8	4.2	
	2	40.0	30.9	11.6	6.0	37.0	10.9	5.7	36.4	11.8	5.4	40.2
	3	14.4	11.1	18.8	8.7	19.4	12.9	9.0	20.6	13.1	9.0	23.8
E	1	129.5		9.1	4.4		5.1	4.4		7.3	3.7	
	2	40.0	30.9	14.3	5.5	38.8	13.2	5.2	35.9	12.9	4.9	40.8
	3	14.4	11.1	20.9	8.3	21.1	12.3	7.3	18.3	15.8	7.3	21.6
F	1	129.5		13.3	4.1		4.9	4.1		9.6	3.3	
	2	40.0	30.9	18.5	4.3	32.8	10.2	4.8	35.9	13.2	4.5	41.5
	3	14.4	11.1	17.2	6.6	17.9	12.1	7.4	20.2	15.6	6.9	22.8

Fuchs, and Gelb and Macleod all agree that individual differences in color constancy are chiefly due to differences of attitude.[9]

All of our *O*s show some degree of regression. Thouless found that each one of 127 *O*s showed some degree of it for size and shape, despite large individual differences. Thouless isolated a group of 14 *O*s conforming to the Kretschmer schizoid type, and 14 more conforming to the cycloid type. He found that the extraverted or cyclothemic subjects showed larger regressive tendencies for shape, size, and brightness.

We turn finally to the effects of the three types of disparation. First of all, it appears that disparation has radical effects on the average phenomenal size. It may be noted from Column 2 of Table IV that the real areas of

[9] Cf. Macleod, *op. cit.*, 97 f.

Series A, B, and C are equal for equal distances. That is, the area of A1 = B1 = C1, etc. This condition obtains also for series D, E, and F. In fact, Series A was formed by merely taking two copies of the left member of the photographs that form Series B. Despite this physical equality of disks A1, B1, and C1, the *O*s see Disk A1 as largest, B1 as smaller, and C1 as smallest. This holds almost uniformly for all series, there being but 4 reversals in 36 possibilities. This tendency to see the same disk as smaller when the disparation increases is manifested in the column "P. reg.," which gives phenomenal areas divided by real areas. Children, for example, see Disk A1 as 6.0 times its normal size, B1 (of the same area and at the same distance) was 5.2 times its real area, and C1 as 4.1 times its real area.

This tendency is probably not due to any influence of the stereoscope. It will be recalled that the pairs of photographs were so mounted that one adjustment of the stereoscope sufficed for all, and that *O* in fact did not make readjustments. Although a number of possible explanations are suggested, we may mention one which seems most plausible. It appears to be a fact that contrast enters into this experiment. We made a new set of slides with india ink and compasses, drawing two circles on a slide corresponding in size to the disks on the 18 photographic slides. Seen in the stereoscope, such areas appear smaller than they really are, as if by contrast with the empty space which surrounds them—like the moon in mid-sky. The disks of the photographs, on the contrary, appear in a space filled with finite objects. But in the slides of Series B and E, and more so with the Slides of C and F, the disparation present gives a slight dimming of vision and the disks seem to shrink in a space which has become somewhat vaster by losing the sharply delineated objects of the slides of Series A and D.

Increasing phenomenal regression with increasing distance exists despite the tendency of phenomenal areas to decrease with increasing disparation (with distance constant). Furthermore, there appears to be evidence that regression actually increases slightly with increasing disparation. If we group together the results for Series A and D, E with B, and C with F, we secure the effects of disparation irrespective of the two sizes of disks used. The results are seen in Table V which gives the average regression associated with the three distances within the three grades of disparation.

Noting the average results for all *O*s in the last column to the right, it appears that the phenomenal areas of position 1 lose more when moved to positions 2 and 3 in the Series A and D, than they do in Series B and E and C and F. The regression increases, in fact, slightly as disparation increases. It will be seen that the differences are small, and that divergencies from the average results occur for the three groups of *O*s. Individual divergencies are

TABLE V

RETENTION OF SIZE FOR THREE GRADES OF DISPARATION
(in percentages)

Disks	Distances	Children	Women	Men	Av.
A and D	2	35.6	35.2	38.6	36.5
	3	18.9	19.3	23.5	20.6
B and E	2	38.0	33.7	40.1	37.3
	3	22.0	18.4	21.7	20.7
C and F	2	37.3	38.4	42.4	39.4
	3	20.7	20.6	23.9	21.7

large in this respect and not worth discussing since the affects of varying disparation are so small for groups.

SUMMARY

The tendency of phenomenal regression to real size (Thouless) was studied in three experiments with two sets of stereoscopic slides, each set made up of photographs of disks of various sizes, photographed at three distances and with varying degrees of disparation. In all three experiments 46 *O*s were used under varying conditions of procedure having in common the requirement that *O* was to gauge the phenomenal areas of the disks seen, whether in or out of the stereoscope, and to select a disk of equal area from a set of comparison circles varying in area. The main results are as follows.

(1) Two types of size-constancy are found: one, a pronounced tendency of *O* to see photographed objects much nearer their real sizes than measurements of the photographs of the objects warrant. The second tendency is the familiar one which leads *O* to perceive a given disk placed at increasing distances as larger than the areas given by the laws of perspective.

(2) In no case is the restoration of real size complete. The phenomenal object is midway between the real physical size of the object photographed and its size as given on the photograph.

(3) Knowledge about the real size of the photograph of the object does not appreciably affect this result. Regression persists despite the presence or absence of knowledge, resembling in this respect ordinary illusions of perception.

(4) When the photographs are viewed without the stereoscope both types of phenomenal regression are reduced but are still present.

(5) Regression becomes somewhat larger with practice, as shown by the course of the reports throughout the experiments.

(6) Sex and age differences are small. Children show the largest and

men the least tendency to see a photographed object as larger than its stimulus-size. Men show the largest and women the least tendency to correct size for increasing distance.

(7) Individual differences are large, but all *O*s show some degree of regression to real size.

(8) Average deviations of individual reports from individual averages are large, but are considerably reduced by knowledge.

(9) Increase in disparation causes a given area at a given distance to appear smaller.

(10) Increase in disparation is associated with a slight increase of the tendency to correct size which has been reduced due to increasing the distance of the object from the observer.

QUANTITATIVE ASPECTS OF THE FLIGHT OF COLORS

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The phenomena known as the flight of colors in the after-images of white light have been observed and described in much detail. The rather extensive literature on the subject covers a period extending from the time of Aristotle to the present.[1] There are, however, only a few general references in this literature to the quantitative relations in the flight of colors. We have been unable to find any systematic account of experimentally controlled observations on the quantitative aspects of the phenomena.

Two quantitative aspects of the flight of colors are (1) the duration of the flight, and (2) the number of the discriminable changes in the flight. The phrase, flight of colors, implies a sequence of phases within the field of the after-image, each phase being qualitatively different from its predecessor, the whole sequence running its course from the beginning of the after-image until it has disappeared from the visual field. The after-image is usually featured by four distinct areas: a center, a narrow ring around the center, a wider edge, and, beyond, a large field extending to the periphery of the visual field. The sequence of changes takes place most prominently within the area of the center of the image, which corresponds in shape to that of the primary stimulus surface. The observations made in the experiments here reported were confined solely to the changes in the center of the image.

Two experiments were carried on to obtain data on the combined effect of the intensity and the duration of exposure of the stimulus light upon (a) the duration of the flight, and (b) the number of phases in the flight. The general plan of the experiments was to make one of two factors a variable,

* Accepted for publication October 2, 1934.

[1] The literature on this subject was reviewed by William Berry in 1922 (The flight of colors in the after-images of a white light, *Psychol. Bull.*, 19, 1922, 307-337); since then the following studies have appeared:

A. M. Shuey, The flight of colors, this JOURNAL, 35, 1924, 557-582; The effect of varying periods of adaptation on the flight of colors, *ibid.*, 37, 1926, 528-537.

W. J. Smith, The law of recurrences and decay of after-images, *Proc. Roy. Soc. Edin.*, 44, 1924, 211-217.

H. Weve, Colors of after-images following strong light-stimulus, *Brit. J. Ophthal.*, 9, 1925, 627-638.

William Berry, Chromatic sequences in the after-images of white light, this JOURNAL, 38, 1927, 584-596.

Ludwig Bayer, Untersuchungen über das farbige Abklingen der Nachbilder mit einem Beitrag zur Theorie des Licht- und Farbensinnes, *Zsch. f. Sinnesphysiol.*, 63, 1932, 197-212.

keeping the other constant throughout. In the first experiment the variable was the intensity of the stimulus, in the second the variable was the duration of the exposure of the stimulus.

Apparatus and Procedure

Both experiments were carried on in the same general physical surroundings. A light proof cabinet was constructed on a long table in the experimental room. The cabinet was 100 cm. long, 70 cm. high and 46 cm. wide. One end of the cabinet was open and *O* sat at the end of the table facing the cabinet. A large, completely light proof enclosure was then pushed up against the cabinet, thus enclosing *O* completely in a small dark chamber. The stimulus was exposed to *O* through an aperture in the far wall of the cabinet.

Experiment I. The object in Experiment I was to use a wide range of light intensities. The initial level of intensity was provided by a 200 watt, gas filled Westinghouse bulb, used in an A.C. circuit of 105 volts. A thin frosted glass was placed between the bulb and the aperture in the cabinet, providing a uniformly illuminated light surface in the field of the aperture. The apparent light surface

TABLE I

Showing the Intensity-Values in Millilamberts per Square Inch of Various Positions of the Wedges

Level	ml.	Level	ml.	Level	ml.
2	2689.9	8	28.8	15	.42
3	907.8	9	15.1	16	.31
4	456.1	10	7.7	17	.23
5	223.7	11	4.0	18	.16
6	112.0	12	2.1	19	.12
7	56.0	13	1.1	20	.09
		14	.6		

was circular in shape and 7.46 sq. cm. in area. Photometric measurement of the apparent light surface gave a value of 23.01 candles per sq. in. The intrinsic "surface brightness"[2] of the illuminated field in the aperture may be expressed in lamberts by means of the formula: lamberts = candles per sq. in. $\times$ 0.487. The result is 11.205 lamberts or 11,205 ml. The distance of the light surface from the nodal points of *O*'s eyes was 193 cm. The size of the retinal area stimulated was approximately 0.049 sq. mm.

The other levels of intensity were obtained by means of a reducing device made up of a pair of Eastman Kodak Neutral Tint Wedges, reciprocally placed and mounted on a track immediately in front of the aperture of the cabinet. The wedges were calibrated according to the formula:[3] density = $\log_{10}$ 1/trans. Observations were made with the wedges placed at 19 different positions giving a transmission range of 2.4×10^{-1} to 8.3×10^{-6} percentage. Table I shows the value in terms of millilamberts for each of the levels.

The duration of the exposure throughout this experiment was kept constant

[2] T. Croft, *Practical Electrical Illumination*, 1917, 65.
[3] Eastman Kodak Co., *Wratten Light Filters*, 1929, 12.

at 60 sec. following a period of dark adaptation of 15-20 min. Six *O*s served throughout the experiment.

After several preliminary attempts to time the phenomenal changes the following method was employed. A kymograph was used with a Marey tambour, which was fitted with a special writing pen.[4] *O*, seated in the dark chamber, held a small rubber bulb which was attached by a long tube to the tambour. As the *O* observed the phenomenal changes he signalled the occurrence of the different phases by sharp pressure on the bulb; this was recorded on the kymographic record by an indented line. The time line, in seconds, was recorded by means of an electric signal marker. *O* also announced verbally the character of the changes or phases he had signalled, and a written record was made of them. By a phase is meant "either the appearance of another color or a gray (achroma) in the area of the image being observed, or the disappearance of the image altogether."[5]

Experiment II. In Experiment II the object was to use a wide range of duration of exposure. The apparent light surface used was produced by illuminating a piece of thin frosted glass through a system of three circular stops placed between the lamp and the shutter used for the purpose of controlling the exposure duration. The intrinsic "surface brightness" of the field in the aperture was 7400 ml., as measured by a Macbeth illuminometer. The area of the surface was 6.63 sq. cm., and was placed at a distance of 126 cm. from the nodal points of *O*'s eyes.

TABLE II

SHOWING THE DURATION OF THE EXPOSURES FOR EVERY ONE OF THE DIFFERENT TIME-INTERVALS USED

Interval No.	Duration (in sec.)	Interval No.	Duration (in sec.)	Interval No.	Duration (in sec.)
1	60	6	5	11	0.1030
2	30	7	3	12	0.0530
3	20	8	1	13	0.0253
4	15	9	0.471	14	0.0117
5	10	10	0.180	15	0.0087

A Zeiss Compur shutter was used to control the duration of the exposure of the stimulus. This shutter is calibrated for automatic exposure for durations below 1 sec. As a check on the accuracy of the automatic 'settings' the shutter was calibrated three times during the course of the experiment.[6] Table II shows the range of exposures used in the Experiment II.

The record of the quantitative relations of the flight of colors in this experiment was obtained by means of a polygraph adapted from a Western Union "call register." In this instrument a ticker tape is automatically drawn over two writing points controlled by magnets. On the tape there was printed side by side the time record, and the phase record as signalled by the S. The time marker was operated electrically by a seconds pendulum swinging through a mercury

[4] The authors acknowledge with gratitude the help given them by officials of the Taylor Instrument Co., who supplied the pens.

[5] Berry, *op. cit.*, 586.

[6] Acknowledgment is gratefully made to Dr. C. W. Fredericks, of the Research Department of the Eastman Kodak Co., through whose courtesy the facilities of the Department were used in making these calibrations.

control switch. The phases were recorded on the tape by a broken printed line, the interruptions in the line being made when *O* pressed the signal key in the dark chamber. Verbal reports of the character of the phases were made at the same time and recorded. Six *O*s served in this part of the experiment.

TABLE III

SHOWING FOR EACH EXPERIMENT AND FOR EVERY LEVEL THE LOG OF THE I. T. PRODUCTS (MILLILAMBERTS × MILLISECONDS), THE AVERAGE DURATION OF THE FLIGHT, AND THE AVERAGE NUMBER OF PHASES IN THE FLIGHT

	Level	Log I.T. products	Av. duration of flight (sec.)	Av. number phases in flight
Experiment I	1	5.82	305.0	32.0
	2	5.21	211.4	30.1
	3	4.73	119.0	12.0
	4	4.43	72.2	10.3
	5	4.12	73.5	11.6
	6	3.82	56.0	8.8
	7	3.53	55.4	7.4
	8	3.23	45.4	5.8
	9	2.95	34.7	5.0
	10	2.66	31.5	5.5
	11	2.38	30.5	5.0
	12	2.10	27.9	4.4
	13	1.82	24.4	4.5
	14	1.56	27.1	5.5
	15	1.38	25.0	5.1
	16	1.27	25.8	4.7
	17	1.14	25.4	4.0
	18	.98	20.1	2.6
	19	.86	18.5	2.5
	20	.73	19.6	2.0
Experiment II	1	5.65	244.0	47.0
	2	5.35	195.9	36.7
	3	5.17	178.0	32.5
	4	5.05	149.0	26.8
	5	4.87	127.0	21.6
	6	4.57	106.0	17.0
	7	4.35	93.0	16.8
	8	3.89	78.0	13.2
	9	3.54	86.4	18.5
	10	3.12	61.7	12.5
	11	2.88	56.6	12.3
	12	2.59	46.6	8.5
	13	2.27	34.3	7.4
	14	1.94	31.0	6.1
	15	1.81	25.5	6.1

RESULTS

The data of the two experiments are so similar in character that they can be readily combined. To do so a common factor is made available in a product established by multiplying the measure of the stimulus by the measure of the duration of the exposure. The first of these terms is available,

in the form of millilamberts, in Table I for Experiment I, and for Experiment II, the measure is 7400 ml. throughout. The second of the terms, in the form of seconds, is available in Table II for Experiment II, and for Experiment I was 60 sec. throughout. The product of these two terms will be referred to as the I.T. product, *i.e.* the product of intensity (in millilamberts) and time (in milliseconds). As the resulting figures are large the

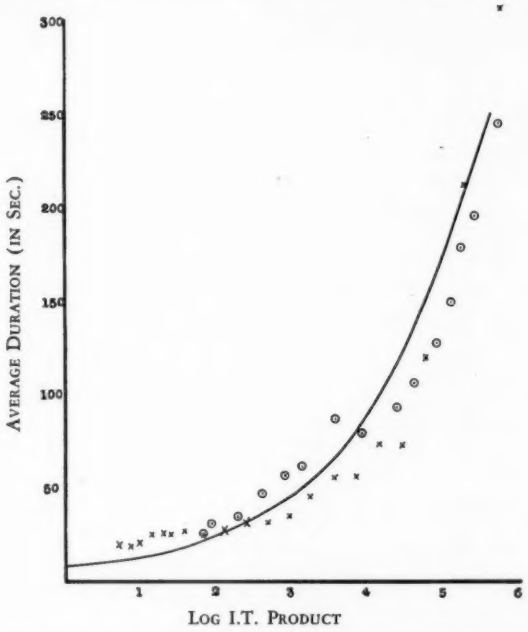

FIG. 1. RELATION BETWEEN THE I.T. PRODUCT AND THE DURATION OF THE FLIGHT OF COLORS

The crosses represent the data from Experiment I and the circles those from Experiment II.

convention of expressing them in terms of their logarithms has been adopted. Table III presents the data.

The number of observations made by the *O*s at the different levels varied somewhat. Owing to the fact that none of the *O*s made more than two observations at one time it was found impossible to make an equal number of observations for each of the levels. The fewest number of observations at any level was 6, the largest number was 41.

The data of Table III are presented in graphical form in Fig. 1. The ordinate of the graph represents the average durations of the flights of colors, in seconds, and the abscissa represents the log I.T. products. The curve drawn through the plotted points was obtained by the method of least squares and indicates an exponential trend of the data. The equation of the curve is $y = 6.51_e{}^{.699x}$.

The close similarity of the two sets of data indicates that the duration of the phenomena of the flight of colors in the after-image is a function of

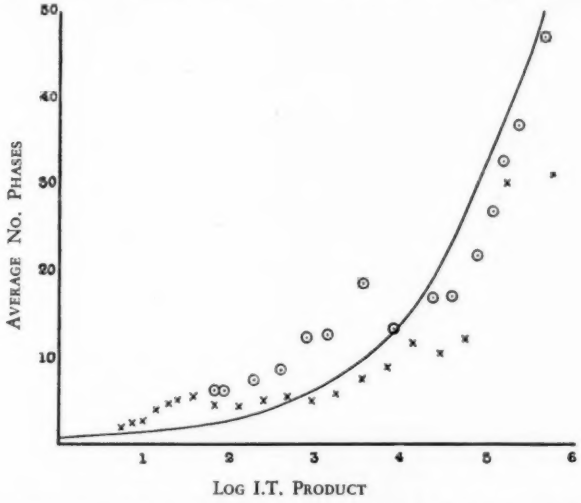

FIG. 2. RELATION BETWEEN THE I.T. PRODUCT AND THE AVERAGE NUMBER OF PHASES IN THE FLIGHT OF COLORS

Crosses, data from Experiment I; circles, data from Experiment II.

the product of the intensity of the stimulus and the duration of its exposure, *e.g.* a relatively low intensity of light stimulus exposed for a period of 60 sec. produced a flight which persisted as long as one produced by a relatively high intensity stimulus exposed for a fraction of a second. As will be seen from an inspection of the table there were no instances where the log I.T. products exactly coincided between the levels in the two experiments. They are, however, in series from the highest to the lowest levels. One level where one of the terms in the I.T. product was common to both experiments was when the duration of the exposure was 60 sec. *i.e.* in Level 1 in both. On the basis of the data presented and within the limits of these

experiments the conclusion is drawn that as the log I.T. product increases arithmetically the duration of the flight of colors increases geometrically. It must, however, be pointed out that in the literature there are some references to very prolonged after-images following the stimulation of the eyes by a very powerful stimulus as, *e.g.* gazing directly into the sun.[7]

The data relative to the number of phase changes in the flight of colors are presented in Fig. 2. The curve drawn on the graph is the best fitting curve, and shows an exponential trend of the data. The equation of the curve is $y = K_{2.85}$, where $K = 0.478$ and $m = 0.814$.

The fit of the experimental data to the theoretical curve is, of course, only approximate. The trend of the data seems, however, to indicate a definite relation between two factors involved, *i.e.* the number of qualitatively different changes in the flight of colors and the I.T. product. As far as we can ascertain this is the first time such a relationship has been experimentally indicated over a relatively wide range. Further experiments are, of course, necessary to test the validity of any generalization based on our data. They seem, however, to warrant the tentative conclusion that as the I.T. product increases arithmetically the number of the phases in the flight increases geometrically.

The trend of the data relative to the duration of the flight and the number of phases shows also a direct relation. In Fig. 3 the data from both experiments are shown with average duration plotted against the average number of phases. The straight line drawn in the graph has as its equation $y = mx$, where $m = 6.07$.

The plotted points cluster quite closely to the straight line, especially at the lower end of the line, corresponding to the lower levels of the I.T. product. At the upper levels there is some divergence from the straight line relation, due quite probably to greater variability in the flights. The most excessive divergence is found in the case where the maximum intensity was used with the maximum exposure time, *i.e.* 11,205 ml. and 60 sec. The divergence in this case is in the direction of the prolongation of the duration of the flight. Although the evidence here is very slight it coincides with other evidence that there is a critical point reached in the intensity of the stimulus light beyond which further increase in intensity is accompanied by a relatively large increase in the duration of the flight. On the basis of the data obtained in these two experiments the following conclusion may

[7] One of the authors has tried the rather hazardous experiment of gazing directly at the bright sun at noon on a cloudless day—an experiment that Fechner tried many times—with the result that an after-image was obtained which lasted for several hours, and in which the colors of the different phases were very brilliant and highly saturated.

be drawn. When a series of different levels of the I.T. product is used, ranging from log product 0.73 to log product 5.82, there is found a direct relation between the duration of the flight and the number of phases in the flight. This relation is quite close through the series from log product

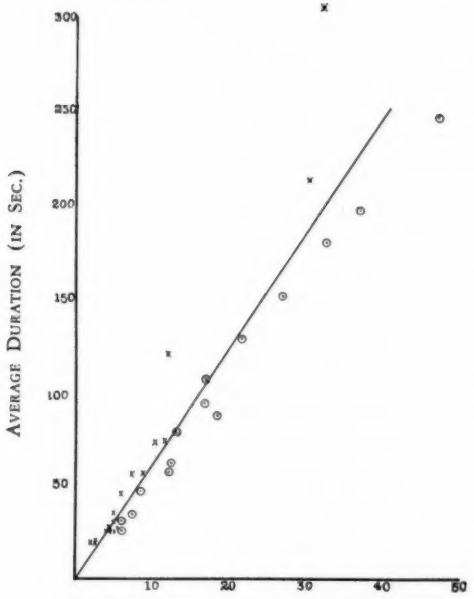

FIG. 3. RELATION BETWEEN DURATION AND NUMBER OF PHASES
Crosses, Experiment I; circles, Experiment II.

0.73 to approximately log product 4.87. Above this point in the series there is some evidence of divergence from the strict straight line relationship.

SUMMARY

To obtain data on two quantative aspects of the flight of colors in the after-image two experiments were independently carried on. In the first experiment the intensity of the stimulus-light was varied from 11,205 to 0.09 ml., while the time of exposure was constant at 60 sec. In the second experiment the duration of the exposure period was varied from 60 to 0.0087 sec., while the intensity of the stimulus was constant at 7400 ml.

The quantitative aspects investigated were (1) the duration of the flight of colors, and (2) the number of phases in the flight.

A common factor in both experiments was the product of the intensity of the stimulus into the duration of the exposure, *i.e.* the I.T. product (millilamberts $\times$ milliseconds).

The conclusions drawn from the results of these experiments are:

(1) As the log I.T. product varies arithmetically the duration of the flight varies geometrically. This applies only within the limits of our experiments.

(2) As the log I.T. product varies arithmetically the average number of phases in the flight varies geometrically.

(3) A corollary of (1) and (2) is shown in the direct relation between the duration of, and the number of phases in, the flight. A close adherence to a straight line relation is indicated from the lowest level used, *i.e.* log I.T. product 0.73 to approximately log I.T. product 4.87. Above this level to the maximum level used, *i.e.* log I.T. product 5.82, some divergence from the straight line relation is seen.

PSYCHOLOGICAL LOSS IN PARETICS AND SCHIZOPHRENICS

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A psychosis usually involves a reduction in the individual's psychological output. Numerous attempts at measuring the loss, especially loss in terms of 'general intellectual capacity,' have been made;[1] but the nature of the loss and the psychological factors responsible for it have not been attacked by experiment. Looking toward a discrimination of various types of loss and toward a determination of underlying factors, the writer has begun in a preliminary way with a series of tasks and procedures.[2] A paradoxical result soon appeared when paretic and schizophrenic patients were presented with a set of arithmetical progressions and a story containing absurdities. Although the work stands at an early stage, we comment here upon this single outcome.

The arithmetical progression was chosen because it presents a problem which contains within itself directions for its solution. Thus explanations and instructions by the experimenter are reduced to a minimum. The progressions employed appear in Table I. They were presented in the order given to 18 paretics and 25 schizophrenics.

Each of the following groups (I-XIII) has its own principle of progression. The members of every group gradually increase in difficulty, as do also the successive groups. Only addition and subtraction are required.

E wrote down a progression as *S* looked on, then placed it before *S* with the comment to "Keep that going. Write down the next two or three numbers in the series." When *S* had finished he returned the sheet and another progression was written and presented. When *S* wrote wrong numbers *E* asked, "Is that right?" But when *S* returned the paper again, it was accepted without comment. Upon reaching Group VII, where the step in difficulty seems to be greater than before, *E* remarked, "You may use your pencil whenever you wish." If, after that, *S* continued to inspect the progression without success, *E* suggested, "Write down the differences." No other verbal instruction was given. *S* was allowed all the time he wished.

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* Accepted for publication June 1, 1934. The paper was read in part before the American Psychological Association in New York City, September, 1934. The author is a National Research Council Fellow in psychology.

[1] For two recent contributions of this type, see C. Simmins, Studies in experimental psychiatry; iv. Deterioration of 'g' in psychotic patients, *J. Ment. Sci.*, 79, 1933, 704; and H. Babcock: *Dementia Praecox: A Psychological Study*, 1933, 1-167.

[2] This work began while the writer was associated with the New York Psychiatric Institute and Hospital and was continued at Worcester State Hospital. The writer expresses his gratitude to both institutions for privileges granted.

TABLE I

THE ARITHMETICAL PROGRESSIONS USED IN THE STUDY

I	II	III	IV
2, 4, 6, 8 . . .	11, 9, 7, 5 . . .	1, 1, 2, 2, 3, 3 . . .	6, 1, 7, 1, 8, 1 . . .
3, 6, 9, 12 . . .	19, 16, 13, 10 . . .	2, 2, 4, 4, 6, 6 . . .	3, 0, 5, 0, 7, 0 . . .
2, 5, 8, 11 . . .	49, 42, 35, 28 . . .	9, 9, 7, 7, 5, 5 . . .	12, 2, 9, 2, 6, 2 . . .
2, 8, 14, 20 . . .	42, 37, 32, 27 . . .	17, 17, 13, 13, 9, 9 . . .	19, 3, 17, 3, 15, 3 . . .
3, 11, 19, 27 . . .	85, 76, 67, 58 . . .	2, 2, 5, 5, 8, 8 . . .	11, 2, 8, 2, 5, 2 . . .

V	VI	VII
3, 5, 4, 6, 5, 7 . . .	17, 1, 16, 2, 15, 3 . . .	2, 3, 5, 8, 12 . . .
1, 2, 5, 6, 9, 10 . . .	19, 2, 17, 4, 15, 6 . . .	30, 23, 17, 12, 8 . . .
2, 6, 8, 12, 14, 18 . . .	20, 5, 18, 10, 16, 15 . . .	1, 12, 22, 31, 39 . . .
13, 11, 10, 8, 7, 5 . . .	1, 20, 5, 17, 9, 14 . . .	30, 29, 27, 24, 20 . . .
21, 18, 16, 13, 11, 8 . . .	2, 27, 5, 23, 8, 19 . . .	1, 3, 8, 16, 27 . . .

VIII	IX	X
1, 3, 5, 9, 13, 19 . . .	2, 3, 5, 2, 6, 11, 5 . . .	1, 3, 6, 11, 19, 31 . . .
3, 6, 8, 13, 15, 22 . . .	20, 22, 24, 20, 16, 22, 28 . . .	2, 3, 12, 28, 50, 77 . . .
40, 39, 36, 35, 31, 30, 25 . . .	30, 29, 27, 30, 26, 31, 27 . . .	1, 3, 8, 19, 39, 71 . . .
1, 10, 11, 19, 21, 28, 31 . . .	1, 12, 10, 18, 16, 21 . . .	
50, 49, 47, 42, 36, 27 . . .	40, 31, 32, 24, 26, 19, 21 . . .	

XI	XII	XIII
75, 74, 71, 66, 57, 44 . . .	2, 3, 7, 10, 17, 23, 34, 44 . . .	20, 22, 9, 23, 18, 21, 11, 24 . . .
3, 4, 8, 13, 23, 34, 41 . . .	3, 4, 6, 10, 11, 16, 26, 30 . . .	30, 31, 19, 30, 11, 28, 6, 25 . . .
2, 3, 9, 16, 27, 40, 56, 75 . . .	3, 8, 14, 21, 26, 29, 35, 44 . . .	25, 13, 16, 5, 23, 11, 28, 7 . . .

The narrative[3] containing absurdities ran as follows:

On a nice March day our club organized a day's outing. Although it had been raining all night, the roads were quite wet and muddy in the morning; but this did not spoil our pleasure. We came through a woods consisting entirely of fir trees. Unfortunately, there were no leaves on the trees as it was still early in the season; how lovely this woods must be in summer time when the trees with their shade protect us against the sun. Then we saw in the distance a rabbit. I ran after it, but as it ran faster than I did, I could overtake it only slowly and finally caught it. I did not hurt it, but let it go again shortly.

Then we came past fields where the farmers were harvesting. At noon we arrived at the village where we intended to stay. About one year ago the village had suffered heavily from a fire; the tower of the church had burned down entirely; in memory of this event, there was a tablet at the place where the top of the tower had been. In a dairy we asked whether we could have milk and cheese, but as they didn't have any at that time they told us they would milk the cows in half an hour and that then we could have plenty of both. Of course, we were glad to wait and then enjoyed it very much. Most of the crowd now remained in the village, but, together with a friend, I made a little jaunt up to a vantage point. We climbed up for half an hour, enjoyed the lovely view and then returned to the village on a path which was even steeper and also continually uphill.

With many games the afternoon passed quickly. We hardly noticed that the shadows grew shorter and shorter and we were surprised when we saw that the sun was setting. We sat down for a while on the shores of a lake. All of a sudden, a dense fog came up from the lake, but it did not spread over a large area;

[3] Taken from Max Hausmann's article, A method to objectively demonstrate thinking difficulties, *Amer. J. Psychiat.*, 13, 1933, 613-625.

it just covered us and all the things close to us disappeared but all the objects in the distance remained distinctly visible. Tired, but well satisfied, we reached home after complete darkness had fallen.

The steps in presenting this narrative were four. (1) *S* was given the narrative and instructed to read the story aloud. When he had finished he was asked, "What do you think of that story? Like it? It is O.K.?" Account was kept of *S*'s behavior and remarks while reading the story, and his answers to the questions above were recorded verbatim. (2) *S* was asked to repeat freely what he had read. Again his words were recorded. (3) *S* was asked to read the story a second time with the instruction, "See whether you can improve the story. Read it, and every time you come to something you would change if you were writing it, tell me. I shall write it as you would have it." (4) Finally, *S* was instructed to look for absurdities. The instruction ran; "Now read the story again and see whether you can find anything foolish, anything contradictory, or anything ridiculous in it."

A comparison of the performances of the paretics and the schizophrenics appears in Table II.

A clear difference in performance as between schizophrenics and paretics is obvious in the table. The schizophrenics solved the numerical problems far better than the paretics did, but they seldom noted an absurdity in the narrative until the fourth step (*i.e.* when they were specifically told to look for something foolish, contradictory or ridiculous). The paretics frequently found absurdities on the first or second reading, that is to say, at Steps 1 and 3.[4] Thus they note absurdities without external direction but they are decidedly inferior to the schizophrenics in the correct continuation of numerical series.[5]

It appears, then, that the psychological loss incurred in our schizophrenic patients differs in character from that incurred in the paretics. How is this difference in the nature of the loss to be explained?

The comparative inferiority of the paretics in the progressions suggests some kind of reduction in 'intellectual capacity.' Either they fail to comprehend the constant numerical interval or intervals (implied in the series presented) or else they fail to add the new members that will preserve and continue the interval or intervals discovered. Nevertheless they possess the kind of alertness required for the detection of contradictory, conflicting, and absurd statements in the narrative read.[6]

Let us consider the respective performances of schizophrenic and paretic. When instructed to read aloud, the schizophrenic usually complies with-

[4] Only two of the paretics failed to note any absurdities. They were extremely deteriorated as is shown by the fact that the only progression they could solve was 1, 2, 3, 4. . . . Paretics who could no longer read were omitted from Table II.

[5] Our two classes (schizophrenics and paretics) were approximately equated for age and pre-psychotic capacity.

[6] The narrative has been presented to a number of non-psychotic persons of varying degrees of psychological maturity. All except one feeble-minded girl found absurdities at the first reading. Most of these 'normal' persons noted more absurdities than the paretics did.

TABLE II

A Comparison of Schizophrenic and Paretic Subjects

	Arithmetic Progressions *Successful Solutions* (w = whole group solved; p = part of group solved)													Narrative with Absurdities *Absurdities found*				
	Schizophrenic Groups of Numerical Series													Steps				
S	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	XIII	I	II	III	IV	Total
Ak	w	w	w	w	p	w	w	p	p								6	6
At	w	w	w	w	w	w	w	w	p							1	9	10
Be	w	w	w	w	w	w	w	p										0
Br	w	w	p	w	p	p	p											0
Co	w	w	w	w	w	w	w	w	p								10	10
Da	w	w	w	w	p													0
Fr	w	w	w	w	w	w	w										3	3
Go	w	w	w	w	w	w	w											0
Ha	w	w	w	w	w	w	w	w	w	w	w	w	w			2	5	7
Hu	w	w	w	w	w	w	w	p									2	2
Ka	w	w	w	w	w	w	w											0
Ln	w	w	w	w	w	w	w	w	w	w	w						2	2
Ni	w	w	w	w	w	w	p							1			3	4
No	w	w	w	w	w	w	w	p										0
Ov	w	w	w	w	w	w	w											0
Pe	w	w	w	w	w	w	p											0
Po	w	w	w	w	w	w	w	p									2	2
Re	w	w	w	w	w	w	w	w	w								9	9
Ro	w	w	w	w	w	w	w	w	w								4	4
Sha	w	w	w	w	w	w	w	p									2	2
Shw	w	w	w	w	w	w	p										1	1
Si	w	w	w	w	w	w	w	w	w								10	10
Su	w	w	w	w	p	p												0
Sz	w	w	w	w	w	w	w	w									2	2
Ta	w	w	w	w	w	w	w	w										0
	Paretic																	
Ab	p																	0
Al	p	p	p											1		1		2
Ca	w	w	w	p	p	p								2		1		3
Cam	w	w	w	w	p	p	p							2		2	2	6
Cap	p																	0
Ch	w	w	w	w	w	w	w	p						5		3		8
Do	w	w	p	p	p	p	p							2		1	4	7
Em	w	p	p													3		3
Fe	w	w	w	w	p	p	p							4		3	3	10
Fl	w	p	p	p	p									1				1
Hn	w	w	w	p	p	p										5	2	7
Je	w	w	p	w	p	p								4		5		9
La	w	w	w	p	p									3		3		6
Ob	w	w	p											1		1		2
Se	w	w	w	w	p	p								2	1	1	2	6
Sm	w	w	w	w										1		1		2
Th	w	w	w	p	p											1		1
Wo	w	w	w	p	p									2		2	1	5

out selective emphasis and without a change of facial expression. When asked subsequently what he thinks of the story, his answers usually con-

cern only his affective reaction to the whole 'outing' situation described. On the other hand, the paretic appears, in spite of his common difficulty in pronouncing words, to consider the import of each sentence. When he comes to the incident of the rabbit he sometimes laughs outright, or he may read it over a second time with the comment, "Something's wrong here."

The request at the second step to re-tell the story obviously startles both groups of patients. The schizophrenics usually succeed better than the paretics.

In the third step—instruction to improve the story—the schizophrenic considers punctuation, paragraphing, and sometimes the use of a word, but almost never does he comment on the logic of the narrative. The paretic attempts to discover what he had left out in re-telling the story, and he also looks for more absurdities.

The fourth step is superfluous for the paretics. Less than half of them noted additional absurdities when they were instructed to look for them. To the schizophrenic who finds absurdities at this stage, it is frequently an occasion for chagrin. Several remarked indignantly "Why didn't you say that was what you wanted?" When asked whether they had previously noted discrepancies, they hesitated. The common answer was, "No, but I should have if you had told me to." This applies only to those schizophrenics who finally found some of the inconsequential and conflicting details.

These differences in the two groups seem too consistent to be accidental and too pronounced to be superficial. They appear to rest upon some fundamental difference in the psychological operations of schizophrenic and paretic. The conditions of the experiment suggest that inducing and governing factors are important in determining the functional differences discovered.

So far as preparatory instructions are concerned, the two problems present a distinct difference. In the numerical completions the instructions which determine performance are *formal* and *occasional*. The formal instruction runs, "Write down the next two or three numbers, etc." The occasional instruction is presented by the progressive arrangements placed before *S; e.g.* the series 2, 4, 6, 8 . . . . gives the 2-interval which is to determine the continuation as 10, 12, etc. Under these combined instructions, the schizophrenics perform relatively well but the paretics relatively poorly (see Table II). To the schizophrenics the arithmetical problem is both understandable and solvable. By the paretic the problem is usually unsolved. Whether the latter outcome is due to the want of the paretics' resources for solving when comprehended remains unknown.

In the narrative with absurdities the instructions are formal and self. The self-instruction is typically added by *S* in the comments 'But this couldn't be,' 'Of *course* the roads would be muddy,' 'If the rabbit ran faster, . . . ,' and the like. The formal instruction does not bear upon these discrepancies (except in Step 4). The *S*s who solved prior to Step 4 would therefore succeed only when (1) the story was integrated into a coherent sequence and when (2) the integrated story brought the comment 'it should be consistent,' or some surrogate for this verbal comment. In fact, specific self-instructions (as illustrated above) may rest solely upon an organic governor which guides the comprehending functions as they run themselves off, refusing to terminate until that which we formulate as 'logical consistency' is attained. The same persistence of function is common in action, where the organism proceeds until the fingers are snugly pressed to the bottom of the glove or the key has been pushed to the very end of the slot in the lock. The same functional persistence appears also in perception when we continue to peer through the fog until we can make out the direction on the road-sign.

In this problem the schizophrenics perform poorly, then, demanding the extra formal instruction of Step 4; "Now read the story again and see whether you can find anything foolish, etc.," and doing only moderately well with this extra aid. But the paretics find many logical lapses and inconsistencies on first and second readings (Steps 1 and 3), *i.e.* without demanding the formal instruction of Step 4.

Now what is it in instruction and government which functionally limits the schizophrenic in this problem? The possibilities are several, and they remain to be teased out by fractionation and by various experimental controls. The defect may lie (1) in temporal integration of the comprehending functions, for the appreciation of consistency in the narrative must involve a ligation in time during which the topic forms. The defect may lie (2) in the limitations of comprehending, which confine the patient to a bare consideration of gross events. It may lie (3) in the want of governing factors which take the place for the paretics of the warning formal instruction of the fourth step. That is to say that the defect may pertain to functional continuity and integrity, to functional range, or to governmental guidance and control.

Whatever the locus of the defects exhibited in schizophrenic and paretic, we have shown that the defects differ in a marked way, under our experimental control, in patients who typify these two kinds of disorder. It now seems to be feasible to press the experimental method further to determine just what kinds of functional and governmental derangement are implied in such performances as we have selected for investigation.

COLOR SENSATIONS PRODUCED BY INTERMITTENT WHITE LIGHT AND THE THREE-COMPONENT THEORY OF COLOR-VISION

By GLENN A. FRY, Washington University

In a previous paper[1] were described the color sensations produced by the intermittent exposure of a small bright white area. The color was found to be pink, purple, blue, or green, depending upon the intensity, duration and frequency of stimulation. It was suggested that these effects could be accounted for on Troland's modulation theory of transmission of color impressions from the retina to the cortex on the ground that intermittent stimulation might modulate the optic nerve-current.

In the present paper[2] the writer points out the difficulties which one encounters if he tries to account for the effects in terms of the three-component theory of color-vision. The explanation offered by this theory is that the three kinds of retinal receptors do not respond with the same relative vigor to intermittent stimulation as to steady stimulation. Since the three kinds of receptors must be supposed to respond selectively to the different regions of the spectrum, measurement of the brilliances produced by intermittent spectral lights should provide data bearing upon this problem.

These brilliances were measured with the apparatus illustrated in Fig. 1. Light emerging from the spectrometer slit was focused by Lens II upon the artificial pupil, which consisted of a horizontal slit 0.64 mm. wide and 1.98 mm. long. Since the image of the spectrometer slit fell vertically across the pupillary slit, slight changes in the relative positions of the two could not affect the quantity of light reaching the eye. The stimulus-pattern consisted of the white card (K) with the two rectangular holes (A and B) which were ½ in. high and ¼ in. wide and ¼ in. apart. The distance from the pattern to the eye was 14 in. The white card was illuminated by the two sources, X and Y, and the holes (A and B) were illuminated from the rear by the light from the spectrometer slit. Disk I cut on the illumination of A intermittently. The on-off ratio was kept constant at 1/5. Hence the quantity of light reaching the eye per unit time remained constant regardless of the rate of intermittence. For the various rates of intermittence of A the brilliance produced was measured by adjusting the brightness of B until A and B appeared equal. Disk II served as an episcotister to regulate the brightness of B. The size of the open sectors could be varied while the disk was in motion. The eye fixated a point midway between the two rectangles. At slow rates the experience is complicated, consisting in a succession of bright flashes separated by dark intervals, but at 6 or 8 per sec., although a fine flicker is still present, a single brilliance value can be assigned. For a given wave-length and intensity of A, and for a given brightness of ground, the brilliances obtaining at different frequencies were measured

* Accepted for publication May 28, 1934.

[1] G. A. Fry, Modulation of the optic nerve-current as a basis for color-vision, this JOURNAL, 45, 1933, 488-492.

[2] The present paper is from the Department of Ophthalmology, Oscar Johnson Institute, Washington University, St. Louis.

at one sitting, in the order from high to low frequencies, with five readings made for each. At 40 per sec. fusion is complete, but, since A and B fall upon different areas of the retina, the on-off ratio of Disk II is seldom exactly the same as that of Disk I when it is adjusted to give equal brilliance, and it may vary from day to day. Furthermore the intensity of illumination of the two holes was shown by the flicker method not to be exactly equal for all wave-lengths. In order to compensate these two factors and to make the data for different wave-lengths comparable, the brightness values of B for each set of readings were multiplied by a factor which

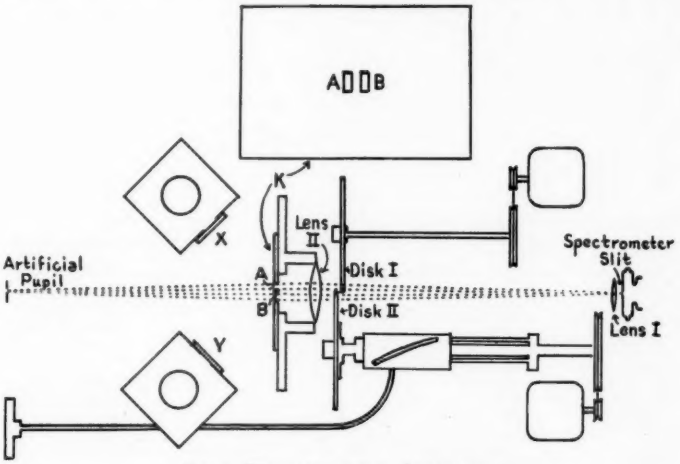

FIG. 1. DIAGRAM OF THE APPARATUS

A is the field whose brilliance is to be measured. Its brilliance can be varied by changing the rate of intermittence (6 to 40 per sec.). The on-off ratio is kept constant at 1/5. B is the measure (always operated at a high rate of intermittence, 60 per sec.) whose brilliance can be varied by changing the on-off ratio.

changed the value for 40 per sec. to 16.5 photons. Hence in the graph in Fig. 3 all the wave-lengths have the same brilliance value at 40 per sec. and the differences at lower frequencies represent the effects of intermittence of stimulation.

One important factor in the stimulus situation is the brightness of the ground. This was shown by the following experiment. The intensity of A was kept constant at 98.8 photons, the on-off ratio at 1/5. The same wave-length, 460 mμ, was used throughout. In the upper graph in Fig. 2 plotted against the frequencies of stimulation of A are the brightnesses of B which produce brilliances equal to A. When the brightness of the surrounding field is low, the brilliance of A increases to a maximum and then decreases, but when the field is above the Talbot (average) brightness of A the hypermaximal phase does not occur.

Different effects are also obtained with differment intensities of A as shown by the lower graph in Fig. 2. The value 540 mμ was used for both curves. The on-off ratio was 1/5. The brightness of the surrounding field was 17.0 photons.

The less intense stimulus shows a relatively smaller decrease in brilliance with the decrease in frequency.

In Fig. 3 the curves represent effects obtained with different wave-lengths of light. The intensity of A was kept constant at 98.8 photons, the on-off ratio at 1/5, and the brightness of the surrounding field at 17.0 photons. The curves are

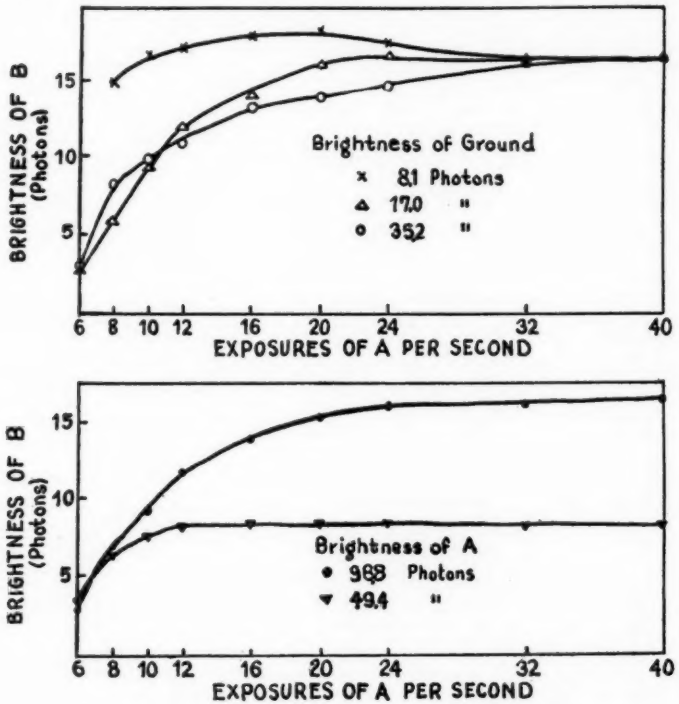

FIG. 2. GRAPHS SHOWING THE EFFECT OF VARYING BRIGHTNESS
The upper graph shows the effect of varying the brightness of the background, while the lower shows the effect of varying the brightness of the intermittent stimulus, A.)

practically identical for all wave-lengths except at the red end of the spectrum where there is a relatively greater diminution in brilliance at low frequencies.

The weak response at low frequencies at the red end of the spectrum can be interpreted in either of two ways. (1) On the one hand, one might assume that the responses of the red, green and blue receptors can be elicited in relative isolation by stimulating with lights from the red, green, and blue regions of the spectrum, and accordingly the data in Fig. 3 might be taken as evidence that the

red receptors react with less vigor to intermittent stimulation than the blue and the green. (2) On the other hand, one might assume that of red, green, and blue light each excites all three kinds of receptors, that the differences in hue associated with the different wave-lengths depend upon minute differences in the degrees of the three responses, and accordingly that the weak response to intermittent red light must signify that all three kinds of receptors respond weakly to intermittent light at the red end of the spectrum. In this respect the red receptors need not be conceived to suffer relatively more than the rest.

Two corresponding explanations could be offered for the production of a blue-green sensation with an intermittent white light. On the one hand, it might be said that the long-wave components of white light are made less effective than the rest by intermittent application to the eye and that the total effect is the same as if the long-wave components were subtracted from the white stimulus. But this possibility is ruled out by the fact that the same results can be obtained with a white light composed of blue and yellow as with a white containing red. This was demonstrated by the following experiment.

A white cardboard screen with a single round hole (½ in. in diam.) was substituted for the one with the two rectangular holes. The brightness of the field surrounding the hole was made 33.4 photons. The hole itself was intermittently illuminated from behind by light emerging from the spectrometer, the on-off ratio being 1/44. A double slit was used at the end of the collimator tube, so that the light emerging from the other end of the spectrometer was a mixture of 26.5 photons of blue (460 mμ) and 1752 photons of yellow (575 mμ), a total of 1778.5 photons. The colors obtained with different frequencies were as follows:

4 per sec.	6 per sec.	8 per sec.	10 per sec.	15 per sec.
White flashes				
Dark purple intervals	Blue	Blue-green	Greenish white	White

As shown below, the results are similar to what is obtained when a white light of the same intensity but containing red light is used instead of the blue-yellow mixture. A milk glass illuminated from the rear by a 250-watt lamp was the source.

4 per sec.	6 per sec.	8 per sec.	10 per sec.	15 per sec.
White flashes				
Dark violet intervals	Blue	Blue-green	Greenish white	White

A second explanation of the production of a blue-green sensation with intermittent white light would be that the red receptors respond more feebly to intermittent stimulation than the green and the blue and leave these two to predominate. This explanation is not affected by the fact that a blue-green sensation is produced with intermittent white light containing no long waves, because regardless of the composition of white light, the red receptors must be equally involved in every case. The crucial factor in the second explanation is *intermittent* stimulation of *red receptors,* whereas in the first it was the intermittent stimulation with *red light.*

Three important facts still need to be accounted for. (1) Blue-green is not

the only color produced with intermittent white light. By changing the frequency a gamut of colors ranging from purple to green can be produced, and according to the three-component theory the relative vigor of the responses of the three kinds of receptors must be supposed to change correspondingly, but the data in Fig. 3 furnish no evidence of this. (2) If the white light is intense enough and the exposures are long enough, a pink sensation can be produced, and under these conditions it must be supposed that the red receptors predominate over the green and the blue. The intensities attainable with the apparatus at hand were not adequate to investigate thoroughly this aspect of the problem. (3) Finally a blue-

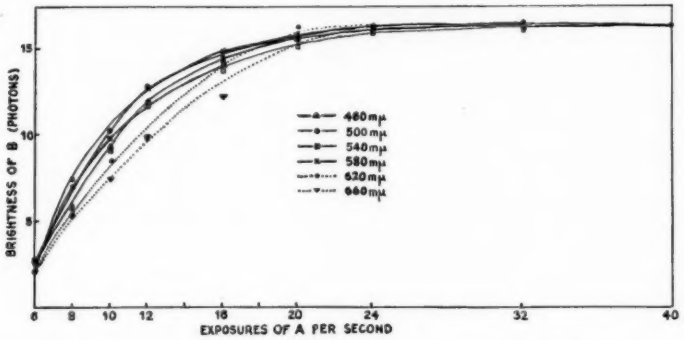

FIG. 3. GRAPH SHOWING THE EFFECT OF VARYING THE WAVE-LENGTH

green sensation cannot be produced with intermittent white light unless the surrounding area is brightly illuminated. This was shown in a previous investigation.[3] The three-component theory offers no ready explanation of this fact. An attempt was made to measure the brilliances produced by different spectral lights when the surrounding field was left dark, but the results were not satisfactory owing to the extreme variability of the readings under these conditions.

The weakness of the visual response to intermittent red light may be related to the weakness of the effect resulting from intermittent exposure of a photographic plate to red light. As in the case of the eye, so also in the case of the photographic plate intermittent exposure is less effective than continuous exposure for all wave-lengths, and the longer waves suffer relatively more than the rest in this respect.[4]

It should be mentioned in connection with this paper that Burch in observing a spectrum of sunlight intermittently presented to the eye with an on-off ratio of 1/4 and a rate of 27 to 54 per sec. found phenomenal dark bands in the regions of yellow, blue-green, and blue-violet. This effect seemed to be associated particularly with the fovea. This sort of phenomenon does not appear in the data

[3] Fry, *op. cit.*, 492.

[4] J. M. Blair and M. C. Hylan, The intermittency effect in photographic exposure, *J. Opt. Soc. Amer.*, 23, 1933, 353-358.

in Fig. 3 and the rates at which it occurs are too fast for it to be related to the production of color-sensations with intermittent white lights.[5]

Summary.—The brilliance produced by intermittent stimulation has been studied with special reference to the effects of varying the intensity, frequency, and wave-length of the stimulus, and the brightness of the surrounding area. It was found at low frequencies that the brilliance produced by stimuli of long wave-length is relatively less than that for other wave-lengths of the same intensity. This may mean either that intermittent *red light* feebly excites the eye, or, in terms of the three-component theory of color-vision, that the *red receptors* respond feebly to intermittent stimulation. Ineffectiveness of the *long-wave* components of intermittent white light cannot account for the production of a blue green sensation with such a stimulus, because this phenomenon is obtained with a white light containing no long waves (*e.g.* a yellow-blue mixture). On the other hand, a feeble response of the *red receptors* to intermittent stimulation might offer a satisfactory explanation, but the dependence of the phenomenon upon the brightness of surroundings, and the variation in color with changing intensity, duration, and frequency of flashes have not been accounted for.

Effects are found in photography which are to some extent analogous to those in the human eye reported in this paper.[6]

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[5] G. J. Burch, An account of certain phenomena of colour vision with intermittent light, *J. Physiol.*, 21, 1897, 426-434.

[6] Acknowledgment is here made to Professor P. W. Cobb, under whose direction this work has been conducted.

AN EXPERIMENTAL COMPARISON OF THE METHOD OF SINGLE STIMULI AND THE METHOD OF CONSTANT STIMULI IN GUSTATION

By CARL PFAFFMANN, Brown University

The investigations of Wever and Zener[1] have shown that the psychophysical method of single stimuli can be employed, with certain advantages, in experiments with lifted weights. The method has also been used satisfactorily in other sense fields—by Pratt,[2] who studied the time error involved in auditory judgments; and by Fry, Haupt, and Wartena,[3] who made a determination of cutaneous spatial thresholds. In this study an attempt is made to ascertain the advantages of the method of single stimuli over those of the method of constant stimuli for experimentation in gustation. The sensitivity of three *S*s to di-sodium hydrogen phosphate (giving rise to a pure saline taste) was determined by both methods.

Procedure. Three *S*s were tested simultaneously at a long table, 8 x 2 ft. The taste samples were presented by *E* from behind a screen extending along the middle of the table. At each of the *S*s' places distilled water was available for rinsing the mouth. For convenience, a trough was constructed along the edge of the table into which the *S*s expectorated.

Six stimulus solutions of 0.10, 0.115, 0.13, 0.145, 0.16, and 0.175 molar concentration and a standard of 0.1375 molar concentration were used with the constant method. Three sets of twelve unmarked drinking glasses contained the solutions. Six glasses contained the comparison stimuli and six the standard. The glasses were arranged behind the screen in such a way that *E* could note the contents of each glass. *E* placed the appropriate pairs of glasses, in addition to a glass containing distilled water, before each *S*. In accordance with standardized directions, *S* took a mouthful of the first solution, rinsed his mouth, then took the second solution and expectorated. Judgments were made in terms of the second solution as greater or less than the first. Five seconds, as indicated by a stop-watch, elapsed during and between each stimulation so that, in all, the double stimulation required 15 sec. In addition, the mouth was rinsed before and after each comparison. Before the next comparison, 45 sec. elapsed during which the next pair of stimuli was placed before the *S*. At this rate, one comparison was made every minute. The standard was presented first in one series and second in the next, and the comparison stimuli were presented in random order in any single series of trials. A total of 50 judgments per stimulus-value were obtained over a period of 5 days.

The method employed here is similar to that reported by Saidullah[4] and

* Accepted for publication May 27, 1934.

[1] E. G. Wever and K. E. Zener, The method of absolute judgment in psychophysics, *Psychol. Rev.*, 35, 1928, 466-493.

[2] C. C. Pratt, Time-errors in the method of single stimuli, *J. Exper. Psychol.*, 16, 1933, 798-814.

[3] F. D. Fry, D. D. M. Haupt and L. Wartena, The D.L. for cutaneous two-point stimulation by the method of single stimuli, *ibid.*, 16, 1933, 743-744.

[4] A. Saidullah, Experimentelle Untersuchungen über den Geschmacksinn mit besonderer Berücksichtigung des Weber-Fechnerschen Gesetzes, *Arch. f. d. ges. Psychol.*, 60, 1927, 457-484.

Kopera[5] except that the solutions were not swallowed. Saidullah, however, has shown that this modification of the procedure has a negligible effect, if any, upon the results obtained. Furthermore, the interval of stimulation (5 sec.) and that elapsing between stimulations (45 sec.) are believed to have been of such magnitude as to prevent adaptation and to permit recovery of the receptor. Mayer[6] has shown that adaptation effects are apparent after 15 sec. of continuous stimulation,

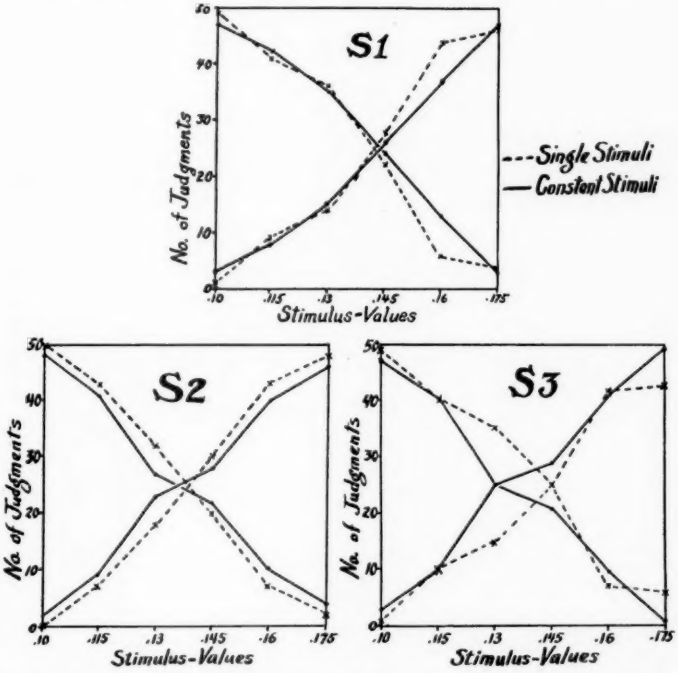

FIG. 1. PSYCHOMETRIC FUNCTIONS FOR THE METHOD OF SINGLE STIMULI AND THE METHOD OF CONSTANT STIMULI. SUBJECTS 1, 2 AND 3.

while Hahn and Günther[7] report that, following a stimulating interval of 6-7 sec., at least 30 sec. are sufficient for complete recovery of the receptor.

The same procedure and stimuli were utilized with the method of single stimuli. In this case, the *S*s were required to assign a number-value ranging from one to

[5] A. Kopera, Untersuchungen über die Unterschiedsempfindlichkeit im Bereiche des Geschmacksinns, *ibid.*, 82, 1931, 273-307.

[6] B. Mayer, Messende Untersuchungen über die Umstimmung des Geschmackswerkzug, *Zsch. f. Sinnesphysiol.*, 58, 1927, 133-152.

[7] H. Hahn and H. Günther, Über die Reize und die Reizbedingungen des Geschmacksinns, *Arch. f. d. ges. Physiol.*, 231, 1932, 48-67.

six (one, least intense; six, most intense) to the single stimuli. Numbers one to three were considered as *minus* judgments, numbers four to six as *plus*. As in the case of the constant method, 50 judgments per stimulus-value were obtained. At the beginning of each experimental period, a preliminary presentation of the entire series of stimuli was made in order to acquaint the *S*s with the range. The *S*s, however, were instructed to begin judging when the first stimulus was presented. All three *S*s were practiced in making discriminations of the sort required.

All experiments were carried out at 5 P.M. The temperature of the solutions was controlled by room temperature, the average value of which was 22.7° C. ±0.7°. Renshaw in a personal communication suggests that temperature fluctuations of one to two degrees centigrade at about room temperature have little effect upon gustatory sensitivity as measured by RL values for sodium chloride.

The data were treated by Urban's process. Since a two-category plan of judging was used, some measure of sensitivity based upon the measure of precision (h) had to be utilized. This value is the probable error (P) of the distribution of judgments, suggested by Culler.[8] It indicates the range in the distribution within which there is equal probability of either a *plus* or *minus* judgment to be given.

Results. In general the results show that the frequency distribution of judgments, the measures of precision and sensitivity, the extent of practice effects and the magnitude of the time-errors are similar for both the single stimulus and the constant stimulus methods.

The judgments obtained from the three *S*s gave frequency distributions closely approximating the normal psychometric function.

The degree to which the curves in Fig. 1 approach normality is indicated in Table I.

TABLE I

SHOWING DEGREE TO WHICH RESULTS APPROACH NORMALITY

$\Sigma(p'-p)^2$

S	Constant Stimuli	Single Stimuli
1	.0038	.0107
2	.0101	.0027
3	.0148	.0143

For a perfect fit, it should be remembered that $\Sigma\ (p'-p)^2$ becomes zero. Reference to the table shows that this value is approximately the same for both methods.

The measure of precision and sensitivity are summarized in Table II.

TABLE II

SHOWING THE MEASURE OF PRECISION AND SENSITIVITY

	S_1		S_2		S_3	
	Constant St.	Single St.	Constant St.	Single St.	Constant St.	Single St.
h	27.7733	31.5467	27.5733	33.4467	29.0067	26.9933
P	.0172	.0151	.0173	.0142	.0165	.0177
PE_P	.0012	.0010	.0012	.0010	.0011	.0012

[8] E. Culler, Studies in psychometric theory, *Psychol. Monog.*, 35, 1926 (no 163), 115-122.

The difference between the measures of sensitivity, P, as determined by each method, is statistically insignificant, as shown in Table II.

TABLE III

SHOWING THE DIFFERENCE BETWEEN THE MEASURES OF SENSITIVITY, TOGETHER WITH THEIR PROBABLE ERRORS

S	Diff. P.E.
1	$.0021 \pm .0016$
2	$.0031 \pm .0016$
3	$.0012 \pm .0016$

Apparently no progressive practice effects were found with either method. The measure of precision, h, derived from judgments fractionated into groups of 25 and 10 judgments per stimulus-value appear in Table IV. The measure of precision is lower for the second group than for the first group of 25 judgments. In addition, no regular increase in h is found for the 5 groups of 10 judgments,

TABLE IV

SHOWING THE AVERAGE VALUES OF h FOR FRACTIONATED DATA

	1st 25	2nd 25			
Constant Stimuli	27.5244	26.0000			
Single Stimuli	31.1822	28.9400			
	1st 10	2nd 10	3rd 10	4th 10	5th 10
Constant Stimuli	21.2558	18.7600	20.8911	19.9067	27.6267
Single Stimuli	25.0155	31.9811	22.7178	21.6138	31.6889

although the fifth group shows consistently high values in each case. It should be remembered that P, the measure of sensitivity, bears an inverse relation to the measure of precision, so that a decrease in h corresponds to an increase in P. If the practice effects are considered in terms of P, reference to Tables V and VI

TABLE V

SHOWING THE VALUES OF P FOR EVERY S FOR THE DATA FRACTIONATED ON THE BASIS OF TWENTY-FIVE JUDGMENTS TOGETHER WITH THEIR PROBABLE ERRORS

	S_1	S_2	S_3
	Constant Stimuli		
1st 25	$.0194 \pm .0019$	$.0154 \pm .0015$	$.0177 \pm .0017$
2nd 25	$.0192 \pm .0018$	$.0197 \pm .0019$	$.0165 \pm .0016$
	Single Stimuli		
1st 25	$.0157 \pm .0015$	$.0138 \pm .0013$	$.0167 \pm .0016$
2nd 25	$.0158 \pm .0015$	$.0148 \pm .0014$	$.0194 \pm .0019$

will show that the differences between the values of P for the data fractionated on the basis for 25 and 10 judgments are insignificant except in one case. Table V gives the probable error of P for the first 25 and second 25 judgments for each S and each method, while Table VI summarizes the lowest and highest

values of the daily variations (groups of 10 judgments) for each method for each *S* and also the calculated range (4 x P.E.) which indicates the overlap in the value of *P*.

TABLE VI

SHOWING THE STATISTICAL RANGE OF VALUES FOR *P* FOR EVERY *S* FOR THE DATA FRACTIONATED ON THE BASIS OF TEN JUDGMENTS

	Constant Stimuli			Single Stimuli	
S	*P*	Range		*P*	Range
1	.0186	.0074–.0298	low	.0121	.0049–.0193
	.0386	.0154–.0618	high	.0259	.0103–.0415
2	.0165	.0065–.0265	low	.0121	.0049–.0193
	.0285	.0113–.0457	high	.0219	.0087–.0351
3	.0156	.0060–.0252	low	.0124	.0048–.0200
	.0264	.0104–.0424	high	.0594	.0234–.0954*
				.0320	.0130–.0512†

* This value does not overlap with the lowest value obtained.
† This is the next highest value to indicate that overlap does occur for all other values.

Fig. 2 indicates the variations in the values of *P* calculated on the basis of 10 judgments. For *S* 3 only, the measure *P* of the first group of judgments is definitely higher than the measure for the subsequent groups. This *S* had, prior to the experiments here reported, just completed a series of judgments by the method of single stimuli in which an intermediate category was employed: 5 stimulus-values were used to which numbers 1 to 5 were assigned. The other *S*s had been trained with the constant method. *S* 3 reported that when he first began the single

TABLE VII

SHOWING THE DIFFERENCE BETWEEN THE POINTS OF SYMMETRY AND THE MID-POINT OF THE STIMULUS-SERIES

S	Constant Stimuli	Single Stimuli
1	.0044	.0025
2	.0007	.0007
3	−.0021	.0046

stimulus-series of 6 stimuli, he found great difficulty in adapting his judgments to a 6-category plan which contained no intermediate category. Upon subsequent trials this difficulty disappeared. The marked decrease in *P* after the first group of judgments corresponds to a practice effect. Fernberger[9] concludes that practice effects appear as a result of progressive improvement in the definition of the categories of report. The report of the single *S* in this case corresponds to this view.

It is believed that the failure of these experiments to disclose general progressive practice effects may be accounted for by the following considerations: (1) the change in sensitivity from day to day may have obscured such effects, (2) the *S*s were practiced in making the required discriminations, (3) a small total num-

[9] S. W. Fernberger, On absolute and relative judgments in lifted weight experiments, this JOURNAL, 43, 1931, 560-578.

ber of judgments were given and an unreliable grouping of this total employed as the basis for the analysis of practice effects. It may be, as Fernberger has shown in experimentation with lifted weights,[10] that, if a greater total number of judgments had been given and larger groupings employed as a basis for analysis, practice effects could have been found.

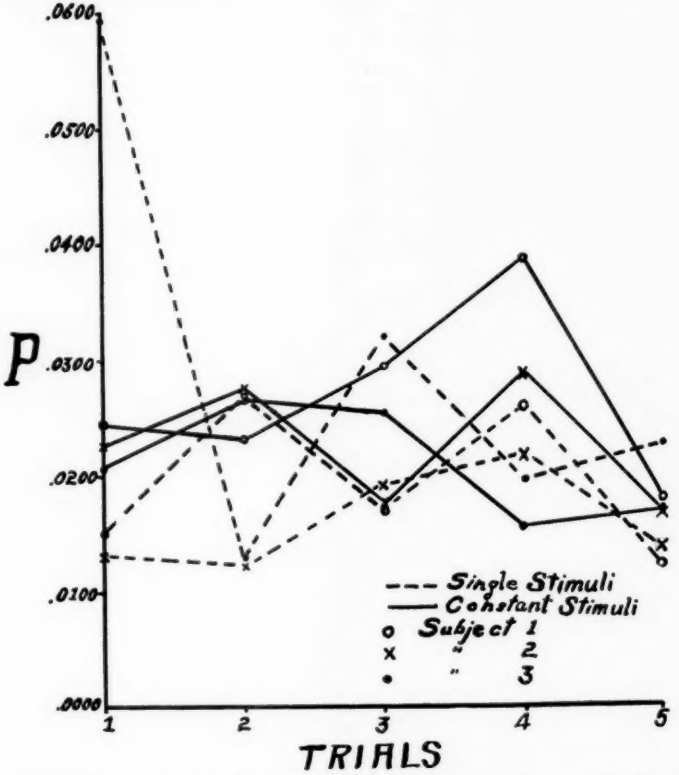

FIG. 2. VALUES OF *P* IN FIVE SUCCESSIVE SETS OF TEN JUDGMENTS EACH FOR THE METHOD OF SINGLE STIMULI AND THE METHOD OF CONSTANT STIMULI WITH SUBJECTS 1, 2, AND 3
(The functions show no practice effects.)

Table VII represents the difference between the point of symmetry and the midpoint of the stimulus-series. Reference to the table shows that the time-error for both methods was of the same general magnitude and in the same direction, with one ex-

[10] Fernberger, The effects of practice in its initial stages in lifted weight experiments and its bearing upon anthropometric measurements, this JOURNAL, 27, 1916, 261-272.

ception. In every case the method of single stimuli yielded a positive time-error. These results are contrary to those obtained with lifted weights,[11] in which the single stimulus-series consistently gave a negative time-error. Fry, Haupt, and Wartena[12] working with cutaneous spatial thresholds found both a negative and a positive time-error. The analysis of the time-error by Pratt,[13] however, adequately accounts for this phenomenon. He found that the presence of an "unconscious standard of reference" set up during the past experience of the individual becomes apparent in the direction of the time-error. Intensive values above this standard of reference were shifted upwards, and low intensities downwards, so that the time-error with the method of single stimuli would tend to be negative at the upper end of the intensitive scale and positive at the lower end. The range of intensities employed in this experiment would thus appear to be low on the intensitive scale for the three *S*s. Actually this range was not far removed from the lower limit of sensitivity, as indicated by preliminary experiments.

Conclusion. Since the results show that the frequency distribution of judgments, the measures of precision and sensitivity, the extent of practice effects and the magnitude of the time-errors are similar for both methods, it is concluded that the method of single stimuli is as adequate and certainly more convenient than the method of constant stimuli for psychological experimentation in gustation.

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[11] E. Davies and W. W. M. Babb, Concerning the absolute method in psychophysics, this JOURNAL, 44, 1932, 367-369. Fernberger, *op. cit.*, 1931; Fernberger, and F. W. Irwin, Time relations for the different categories of judgments in the absolute method in psychophysics, this JOURNAL, 44, 1932, 505-525; Wever and Zener, *op. cit.*

[12] Fry, Haupt, and Wartena, *op. cit.*

[13] Pratt, *op. cit.*

SIMILARITY IN THE FORM OF ORIGINAL AND INTERPOLATED LEARNING AND RETROACTIVE INHIBITION

By R. H. WATERS and ZILLAH E. PEEL, University of Arkansas

Since the concept of retroactive inhibition was first introduced by Müller and Pilzecker the similarity factor has been the subject of numerous investigations.[1] Interest in this factor has been given a renewed impetus by the more recent paper of Robinson.[2] The investigations on this topic may be conveniently grouped under three heads on the basis of the kinds of similarity dealt with: first, formal or structural similarity of materials; secondly, logical similarity or similarity of meaning; and thirdly, similarity in form of interpolated activity. This is only a rough classification but it will serve to set off rather clearly one type of investigation from another.

The experiment reported in this paper belongs in the last category. Earlier investigations which belong in the same category are those of Müller and Pilzecker,[3] DeCamp,[4] Webb,[5] Robinson,[6] Skaggs,[7] and McGeoch.[8] As a result of these experiments the general conclusion has been reached that more retroactive inhibition is found when the original and interpolated activities are similar to each other. In these investigations either the same form of learning, or a type of activity involving a change from that obtaining in original learning as well as a change in the formal characteristics of the material, filled the interpolated interval.

In the present investigation the effort was made to keep the formal characteristics of the material constant and at the same time to change the interpolated activity in form only, not in general type. More precisely stated the present investigation is to be viewed as being offered as a further test of the conclusion that similarity in form of activity is a factor in retroactive inhibition. Our particular hypothesis was that if the interpolated learning activity differs in form only from the original, less retroactive inhibition will be found than when its form remains the same. For this investigation similarity in form of learning was interpreted as meaning that the original and interpolated material required the same type of learning activity on the part of the subject.

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* Accepted for publication July 12, 1934.

[1] G. E. Müller and A. Pilzecker, Experimentelle Beiträge zur Lehre vom Gedächtnis, *Zsch. f. Psychol.*, Erg. 1, 1900, 179.

[2] E. S. Robinson, The 'similarity' factor in retroaction, this JOURNAL, 39, 1927, 297-312.

[3] Müller and Pilzecker, *op. cit.*, 174-198.

[4] J. E. DeCamp, A study of retroactive inhibition, *Psychol. Monog.*, 19, 1915, (no. 84), 1-69.

[5] L. W. Webb, Transfer of training and retroaction, *ibid.*, 24, 1917, (no. 104), 1-90.

[6] E. S. Robinson, Some factors determining degree of retroactive inhibition, *ibid.*, 28, 1920, (no. 128), 1-57.

[7] E. B. Skaggs, Further studies in retroactive inhibition, *ibid.*, 34, 1925, (no. 161), 1-60.

[8] J. A. McGeoch, The influence of four different interpolated activities upon retention, *J. Exper. Psychol.*, 14, 1931, 400-413.

The two forms of interpolated learning were paired associate learning and serial learning. Two separate experiments were run employing these two forms as original learning. For both experiments three interpolated conditions, rest, serial learning, and paired associate learning, filled a 6-min. interval between learning and relearning as is the customary procedure in retroactive inhibition experiments. The following outline will place the procedure of the two experiments before the reader.

Experiment I	Experiment II
Original learning 5 presentations of 20 pairs of words, followed by immediate recall.	*Original learning* 10 presentations of a list of 20 words, followed by immediate recall.
Interpolated 8 presentations of 20 pairs of words with immediate recall; or, 8 presentations of a list of 20 words with immediate recall; or, 6 minutes spent in reading Judge.	*Interpolated* Same as in experiment I.
Relearning Complete the learning of the original pairs to 1 perfect repetition.	*Relearning* Complete the learning of the original list of 20 words to 1 perfect repetition.

Some further comments may be in order to clarify more completely the experimental procedure. In Experiment I the *S*s were instructed to learn the original and interpolated pairs of words so that they could give the second or response-word later when only the first or stimulus-words would be presented in a different order. In the interpolated serial learning of Experiment I the *S*s were allowed to learn and recall the words of the lists in any order. This method, instead of the anticipation method, was employed because it made the serial learning more unlike the paired associate learning. All materials were presented on a memory drum set for a 2-sec. exposure. In Experiment II, 10 instead of 5 presentations of the original material were given, because some preliminary work showed an insufficient amount of learning following 5 presentations of the lists of words. In Experiment II, in both original and interpolated learning of the lists of words, the anticipation method was used since in this case we wanted one interpolated learning activity to be similar in form to the original.

All materials in both experiments were arranged so that a given set of 20 words appeared equally often as response-words in the original group of paired associates, as original word-lists, as response-words in interpolated paired associate learning, and as interpolated lists of words for serial learning. That is, if a given set of 20 words appeared as original serial learning for one *S*, this same list appeared as interpolated learning for a second *S*, as the response-words in the original pairs of words in Experiment I for a third *S*s, and finally, as the response-words in the interpolated paired associate learning. Thus any effect which might be caused by slight differences in materials was equalized for both experiments and for all conditions.

The question may be raised concerning the low degree of learning employed in this investigation. We suspected a low degree of retroactive inhibition to be manifested by the change in the form of learning in our interpolated conditions and

hence considered it necessary to arrange the conditions in such a way as to allow the retroactive effect, if any, to manifest itself. McGeoch has shown that retroactive inhibition is greater with low degrees of learning, therefore we used a low degree of learning in this investigation.[9] It remains a separate problem to determine whether or not results differing from those obtained in our investigation will be found with higher degrees of learning.

A different group of 24 students from classes in psychology served as *Ss* in each experiment. All the *Ss* were given preliminary practice prior to the beginning of the main experiment. The interpolated conditions were given in a counterbalanced order in both experiments. The experimental sittings extended over 3 days, every *S* going through one condition each day.

Two methods of measuring retention after interpolated learning were employed. Recall scores were computed in terms of the number of items, whether in serial or paired associate learning, correctly given on the first relearning presentation; and relearning scores were computed in terms of the number of trials taken to relearn the original material to one perfect repetition. Due to the fact that the learning of the original material was incomplete, as has been stated above, these relearning scores are not true relearning scores but rather indicate the number of trials required to complete the learning.

TABLE I

AMOUNT OF RETROACTIVE INHIBITION

	Experiment I				Experiment II			
Interpolated conditions	R_I DD	R_I diff.	R_{II}	R_{III}	R_I DD	R_I diff.	R_{II}	R_{III}
Serial learning	.04	.51	20.4%	.4%	4.25	5.9	44.2%	38.3%
Paired learning	1.22	1.51	34.8%	13.3%	.87	1.03	16.2%	7.08%

Three methods of measuring retroactive inhibition in terms of the recall scores were used: Recall I (R_I), the absolute amounts of retroactive inhibition were computed by subtracting the recall score after work from the recall score after rest; Recall II (R_{II}), computed by the formula, $(R_1\text{-}R_r)/R_1$, in which R_1 is the recall on the last learning presentation and R_r is the recall of the first relearning presentation; Recall III (R_{III}), computed by the formula, $(R_r\text{-}R_w)/R_r$, in which R_r is the recall after rest, and R_w is the recall after work. Of these three methods the last two give relative amounts of retroactive inhibition, and of these two Recall II is perhaps the more significant on the grounds that it takes into consideration the degree of original learning. The results as measured by these three methods are given in Table I. In interpreting the results it must be remembered that the original learning in Experiment I consisted of paired-associate learning (words), and in Experiment II of serial learning (list of words).

If our original hypothesis is to be supported, viz., that if the interpolated learning activity differs in form only from the original less retroactive inhibition will be found than when its form remains the same, then the amount of retroactive in-

[9] J. A. McGeoch, The influence of degree of learning upon retroactive inhibition, this JOURNAL, 41, 1929, 252-262.

hibition under the serial learning interpolated in Experiment I must be less than that under paired learning, and similarly, the amount of retroactive inhibition under paired learning interpolated in Experiment II must be less than that under serial learning. A glance at the data of Table I shows that this is exactly what happens in all three comparisons for each experiment. The data of this table show a greater amount of retroactive inhibition when the interpolated learning activity is similar to the original.[10]

Another indication of the effect of the interpolated conditions upon retention is given through or in terms of the number of trials required to complete the learning of the original material. These figures are given in Table II. The data of this table show a slight tendency for fewer trials to be necessary when the form of the interpolated learning is similar to the original. In Experiment I, furthermore, there is a tendency for more relearning trials to be required after rest than after either of the two work conditions. This latter is true in Experiment II only for the interpolated serial learning. This finding is just the reverse, in implication, of that

TABLE II

MEAN NUMBER OF TRIALS TO COMPLETE LEARNING

Interpolated conditions	Experiment I		Experiment II	
	Mean	Sigma	Mean	Sigma
Rest	10.54	1.51	12.45	1.77
Serial learning	8.25	1.29	11.62	1.80
Paired learning	7.83	1.08	12.75	1.80

shown when the retroactive inhibition was measured in terms of recall scores in Table I.[11] It is also out of harmony with the findings of other investigations. It may be a chance finding, of course, but it suggests that interpolated learning, similar in form to the original, may influence retention detrimentally in terms of recall but at the same time facilitate relearning. This second effect of interpolated similar learning may be due to the fact that the interpolated learning serves to maintain the set, or enables the individual to reacquire the set more quickly for the same form of learning.

Theoretically the results of this experiment could be accounted for in terms

[10] That this tendency is due to the factor of interpolated activity and not to differences in degree of learning is shown by the fact that RII, in which degree of learning is taken into account shows no different tendency than do RI and RIII. That differences in retroactive inhibition cannot be referred back to differences in degree of original learning is also shown by the number of items recalled in original learning. For Experiment I these figures are 10.4, 11.4, 12.1 for the rest, serial, and paired learning conditions respectively. In Experiment II the corresponding figures are 12.0, 12.3, and 12.1. These figures indicate small but unreliable differences between the degrees of learning for the three conditions in both experiments.

[11] Since the number of learning presentations in each experiment was constant for each condition, and since the conventional methods of computing saving scores in terms of relearning trials would, in this particular instance, yield negative values, the effect upon retention of the three interpolated conditions can just as truly be seen from the raw figures showing number of relearning trials required to complete the learning to one perfect repetition under each condition.

of either the perseveration doctrine or the transfer hypothesis. In the case of the first it would be necessary to assume that when the original and interpolated learning activities are similar in form the same or in part the same neural mechanisms are involved and hence the 'setting-in' process would be interfered with to a greater extent than when the interpolated learning activity differs in form from the original. In the case of the transfer hypothesis it might be assumed that the maintenance of the same general 'set' brought about by the original learning and interpolated learning similar in form to the first causes a greater confusion of the items in the original and interpolated lists. If, however, the finding that an interpolated activity (when similar in form to the original) facilitates relearning the original material is substantiated by further investigation it would seem to point to the conclusion that an adequate interpretation of retroactive inhibition must be couched in terms of a general set or attitude, rather than in terms of particular and specific neural elements or processes.

SUMMARY

In summary, then, this investigation was undertaken as a further check on the hypothesis that interpolated activity similar in nature to the original will result in greater retroactive inhibition than if the interpolated activity differs in form from the original. In two separate experiments in which the paired associate learning of words was the original learning in the first and the learning of a serial list of words was the original in the second with interpolated conditions of rest, paired associate learning and serial learning it was found that:

(1) By three measures of retroactive inhibition in terms of recall scores there is a greater detrimental influence upon retention when (a) the learning of paired associates is followed by interpolated learning of paired associate words than when followed by the learning of serial word lists, and when (b) the learning of a list of serial words is followed by the learning of serial words than when followed by the learning of paired associates. In other words that similarity in form of interpolated learning is a factor causing increased amounts of retroactive inhibition.

(2) In terms of the number of trials necessary to complete the learning of the original material, when original learning is followed by interpolated learning similar in form to the original, fewer trials are necessary to complete the learning than when the original learning is followed by interpolated learning of a different form. In other words it is suggested that similarity in form of interpolated learning may be a factor which will facilitate relearning.

(3) The results may be interpreted in terms of either the perseveration or transfer theories although our second conclusion suggests that an explanation of retroactive inhibition should perhaps be couched in terms of a general set or attitude rather than, as has been the more prevalent customs, in terms of particular and specific neural elements or processes.

INFLUENCE OF HUNGER ON THE PECKING RESPONSES OF CHICKENS

W. A. BOUSFIELD, Tufts College, and E. SPEAR, Harvard University

It is the purpose of this report to present certain observations of a quantitative nature concerning the influence of hunger on the well organized reflex of pecking in the case of chickens. According to Bayer the energy with which a chicken pecks is correlated with the strength of hunger.[1] The importance of this observation lies in its demonstration of the reflex facilitation of drive, or, more specifically, the facilitation of the consummatory responses. Bayer's finding followed from results obtained by using a unique method to record the energy of pecking. A plate of putty, over which corn was strewn, was placed before a chicken. Bayer noted that deep holes were made by a hungry chicken in pecking, while with less hunger the holes were shallower; but he tells of no actual measurements made of his records. The present experiment, undertaken in the Harvard Psychological Laboratory, was designed to secure the needed quantitative indices by means of a refinement of Bayer's method.

Modelling clay of uniform quality was placed in tin pans 15 cm. square and 1 cm. deep, and then rolled until smooth with the edges of the pans. The surfaces were marked in 1.7-cm. squares by depressing equally spaced parallel wires into the clay. Wheat kernels of approximately uniform size were lightly pressed into the clay at the intersections of seven rows and seven columns, making a square pattern of 49 kernels in the center of each pan. (Cf. Fig. 1.) The pans, prepared in this way, were presented to the chickens, and the time required for pecking up all the grain was recorded with a stop-watch. Each chicken was confined in a cage by itself during the tests. The procedure consisted of first obtaining a control record from one to two hours after the *S* had been removed from the group where a supply of scratch feed was continuously available. After obtaining the control record, the chicken was left in the experimental cage with only water available until the following day, so that a fast of 22 to 24 hr. was imposed. The same procedure was used as with the control, and a second record was secured, after which the *S* was returned to the group. Samples of these records are presented in Fig. 1. In order to secure permanent impressions of these records, plaster of Paris casts were made by pouring a light mixture of the plaster over the surface of the clay.

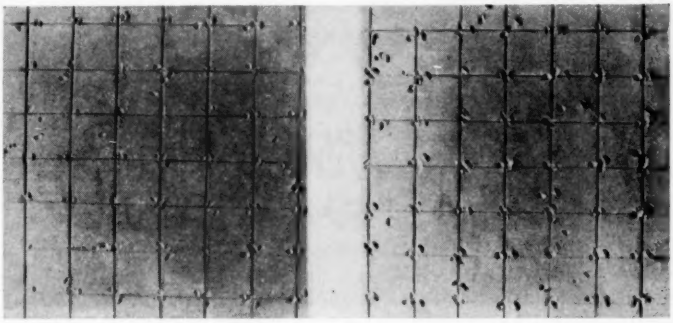
The procedure afforded the following measures: (1) average time per peck; (2) an index of the energy of pecking; (3) the percentage accuracy of pecking. The first of these measures was computed by dividing the total time of the record by the total number of pecks registered. The index of the energy of pecking was determined by measuring the heights of the negatives of the indentations on the plaster casts by means of a spherometer. The percentage accuracy of pecking was determined by multiplying the ratio of the number of kernels to the number of pecks by 100. Typical results from 5 chickens are presented in Table I. In spite

* Accepted for publication June 12, 1934.

[1] E. Bayer, Beiträge zur Zweikomponententheorie des Hungers, *Zsch. f. Psychol.*, 112, 1929, 1-54.

of the individual variations, the trends of the data for each measure are uniform. Bayer's observation of the increased energy of pecking with hunger is thus confirmed. But, we may go further and note that the speed of pecking is increased by fasting, and that the accuracy of the response is simultaneously diminished.

The general facilitation of somatic reflexes that accompanies fasting and the correlated alimentary contractions has been noted by several investigators. The experiments of Wada[2] with human *Ss*, Rogers[3] with pigeons, and Richter[4] with rats show that hunger or the associated contractions are accompanied by an increase in gross bodily activity. Wada's *Ss* were also able to exert a stronger pull



CONTROL DELAY

FIG. 1. RECORDS SHOWING THE INDENTATIONS IN MODELLING CLAY PRODUCED BY THE PECKING OF THE CHICKENS

The 'control' record was obtained after a fast of 1 hr., and the 'delay' record was obtained after a fast of 22 hr.

on a dynamometer during the periods of hunger pangs. Warner's[5] experiments with the obstruction-apparatus indicate that the number of crossings made by rats over an electric grill at first increases with the extent of fasting, and then finally decreases. Carlson[6] found that for human *Ss*, the patellar reflex is increased during periods of stomach contractions. That hunger causes the nursing reflexes of infants to be more readily elicited was noted by Watson.[7] These results uniformly indicate a general facilitation of somatic reflexes during all but extreme stages of hunger.

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[2] T. Wada, An experimental study of hunger in its relation to activity, *Arch. Psychol.*, 8, 1922, (no. 57), 1-65.

[3] F. T. Rogers, The hunger mechanism in birds, *Proc. Soc. Exper. Biol. & Med.*, 13, 1916, 119-121.

[4] C. P. Richter, Animal behavior and internal drives, *Quart. Rev. Biol.*, 2, 1927, 307-343.

[5] L. H. Warner, A study of hunger behavior in the white rat by means of the obstruction method: A comparison of sex and hunger behavior, *J. Comp. Psychol.*, 8, 1928, 273-300.

[6] A. J. Carlson, The influence of the contractions of the empty stomach in man on the vaso-motor center, on the rate of heart beat, and on the reflex excitability of the spinal cord, *Amer. J. Physiol.*, 31, 1913, 318-327.

[7] J. B. Watson, *Psychology from the Standpoint of a Behaviorist*, 1924, 259 f.

The results of the present paper imply that a more exact description of the somatic reflex facilitation of hunger is possible. The increase in the energy and speed of reflex activity are different manifestations of the same underlying physiological condition of the animal. Functional changes of the nervous system, such as alterations in conduction across synapses may be legitimately invoked to account for these two changes. The decrease in the accuracy of pecking correlated with fasting, however, must be regarded as essentially a secondary effect of the wide

TABLE I

TYPICAL DATA FROM 5 CHICKENS SHOWING THE EFFECT OF FASTING ON THE SPEED AND ENERGY OF PECKING, AND THE ACCURACY OF THE RESPONSE

Chick	Average time per peck		Percentage accuracy of pecks		Energy of pecks (depth of indentations)	
	Control	Delay	Control	Delay	Control	Delay
No. 1	0.49 sec.	0.42 sec.	85%	64%	0.31 cm.	0.55 cm.
No. 2	0.59 sec.	0.49 sec.	72%	64%	0.27 cm.	0.57 cm.
No. 3	0.38 sec.	0.37 sec.	86%	74%	0.22 cm.	0.42 cm.
No. 4	0.46 sec.	0.35 sec.	91%	75%	0.27 cm.	0.52 cm.
No. 5	0.45 sec.	0.40 sec.	90%	89%	0.38 cm.	0.42 cm.

spread facilitation. Accuracy of motor response would necessarily imply a circumscribed pattern of excitation with facilitation and inhibition nicely balanced. The widespread facilitation of hunger would upset this balance with a consequent interference with the quality of muscular coördination.[8]

More evidence is needed before an adequate description of the underlying mechanisms involved in hunger can be made. The present study is tentative and suggests that further experimentation on behavorial changes correlated with drive can be made with profit.

[8] A further discussion of the relation of facilitation and inhibition to the hunger drive is given by M. H. Elliott and W. A. Bousfield, Analysis of the hunger drive. (In preparation.)

DEPTH PERCEPTION AND INTERPUPILLARY DISTANCE AS FACTORS IN PROFICIENCY IN BALL GAMES

By BRANT CLARK and NEIL WARREN, University of Southern California

It has been known since the time of Helmholtz that the accuracy of depth perception may be increased by the use of such instruments as the range finder which in effect increase the interpupillary distance of the observer. Depth effects of stereoscopic pictures may also be improved by artificially increasing the interpupillary distance of the observer. This is done by increasing the angular separation of the cameras which record the stereograms, and in certain cases of aerial photography the two pictures may be taken from points as far as 200 feet apart. Such stereograms produce stereoscopic effects which are definitely enhanced. These facts indicate the importance of very wide interpupillary distances in depth perception and stereoscopic vision.

Banister and Blackburn have also suggested that the interpupillary distance is a factor which affects depth perception to a sufficient degree to influence a man's success in sports which involve batting and throwing balls.[1] A comparison was made of the interpupillary distances and athletic ability of 258 Cambridge University undergraduates. The group of men rated (by 4 undergraduate associates) as being 'good' in athletics had a wider average interpupillary distance than the group rated 'poor' in sports. The authors concluded that "this is probably due to the better stereoscopic vision which the greater width makes possible." Banister and Blackburn point out that their method of rating the men's athletic ability was affected by bias, halo effects, and, in certain cases, lack of complete information by the rater.

It was the purpose of the present investigation to determine the effect of interpupillary distance upon depth perception, using a method of rating which was more objective than that used by Banister and Blackburn. This was done in two ways: (1) the scores on a standard test of depth perception were compared with interpupillary distance; (2) the interpupillary distances and depth perception scores of athletes and an unselected group of men were compared.

Method and procedure. The depth perception of the *S*s was measured by using a modification of a test described by Howard.[2] The instrument, known as the Howard-Dolman apparatus, was constructed according to specifications set by the army and navy air services.[3] It consisted essentially of 2 vertical rods, one fixed and one movable. The *S*'s problem was merely to adjust the variable rod into a position so that both rods were the same distance from him. This was done by pulling a heavy cord. He was first permitted to observe them when they were in the correct position, and then the movable rod was set approximately 18 in. back of the fixed

* Accepted for publication July 15, 1934.

[1] H. Banister and J. M. Blackburn, An eye factor affecting proficiency at ball games, *Brit. J. Psychol.*, 21, 1931, 382-384.

[2] H. J. Howard, A test for the judgment of distance, *Am. J. Ophthalm.*, 2, 1919, 656-675.

[3] Specifications were obtained from the Navy Air Service.

rod. Ten trials were taken for every *S*, 5 with the variable rod placed in back of the fixed one, and 5 with it in front. The average of these 10 trials was taken as *S*'s score.

S was not permitted to move his head during the test, and was allowed to pull in one direction only. If he pulled the rod beyond the zero position and so reported, the trial was not counted and he repeated it. All of the tests were made in a well-lighted room with the *S* placed 20 feet from the fixed rod.

The interpupillary distance for infinity fixation was measured by means of an interpupillary distance gauge.[4] The visual acuity of the *S*s was measured by the use of standard Snellen test types placed at 20 ft.

All of the *S*s were male students in the University of Southern California. The Howard-Dolman test was administered to 598 men. Of these, 456 were tested for depth perception only. These data were collected as part of the routine medical examination given to entering students. The group was unselected except that the majority of the men were freshmen in the University. Another group of men was tested for depth perception, interpupillary distance, and visual acuity. Of this group, 103 men having 'normal' vision (20/20 in each eye) were selected for a comparison of interpupillary distance and depth perception. A group of 39 athletes was tested also. Of this group there were 8 men from the tennis team, 13 from the basketball team, and 18 men from the baseball team. As representatives of the University on Varsity teams, these men comprised a highly selected group having superior ability in sports involving throwing and batting balls.

Results. (a) Interpupillary distance and depth perception. In order to determine the relation of interpupillary distance to the perception of depth, the data from the 103 *S*s were examined. Only those *S*s who had normal vision were included in this group, in order to eliminate an important variable affecting depth perception. The correlation of the interpupillary distance and the Howard-Dolman scores was found to be $+0.124 \pm 0.067$. This finding indicates that there is no significant relationship between these two variables. There is a slight tendency, however, in the direction indicated by Banister and Blackburn.

(b) Comparison of the interpupillary distance of athletes and an unselected group. The average interpupillary distance of the group of 103 unselected men was determined, as well as that of the 39 athletes. The average interpupillary distance was found to be:

Unselected men	65.1 ± 0.2 mm.
Athletes	64.5 ± 0.6 mm.
Difference	0.6 ± 0.6 mm.

In the groups tested, the unselected men had a slightly wider interpupillary distance than the athletes, although the difference was not reliable. On the basis of these findings it may be stated that there was no significant difference between the interpupillary distance of athletes and an unselected group of men.

(c) Comparison of the depth perception of athletes and an unselected group. The Howard-Dolman scores of the 456 unselected men and those of the 39 athletes were also compared to determine whether or not the athletes were superior in depth perception. The average error of the two groups was found to be: unselected

[4] Bausch and Lomb Optical Company, Rochester, N.Y.

men, 30.5 ± 1.1 mm.; athletes, 29.3 ± 3.8 mm.; yielding a difference of 1.2 ± 3.9 mm. These results lead to the conclusion that the depth perception of a group of athletes, as tested by the Howard-Dolman apparatus, was not significantly superior to that of an unselected group.

Summary and Conclusions

A total of 598 undergraduate students were tested to determine the importance of interpupillary distance and depth perception in sports. Tests of depth perception were made using the Howard-Dolman apparatus. The interpupillary distance was measured by an interpupillary distance gauge, and the visual acuity was tested by the use of a standard Snellen chart. The results of these experiments lead to the following general conclusions:

(1) There is no significant relation between interpupillary distance and depth perception as measured by the Howard-Dolman test.

(2) The interpupillary distance of athletes does not differ reliably from that of an unselected group.

(3) The depth perception of a group of athletes as measured by the Howard-Dolman apparatus does not differ reliably from that of an unselected group.

The third conclusion raises an important question in relation to depth perception as measured by the Howard-Dolman apparatus. One of two inferences may be drawn from this finding. Either depth perception as measured by the test is relatively unimportant in ball games of the nature of those included, or the test does not give an accurate measure of depth perception. In either event, the application of the results of this test to practical situations may be seriously questioned.

A COMPARISON OF THE RAT'S FIRST AND SECOND EXPLORATIONS OF A MAZE UNIT

By WAYNE DENNIS, University of Virginia

In two previous studies of the maze-exploration of rats we used multiple-unit mazes.[1] Because of this fact the rat could not explore the maze a second time within a short period of time. It seems likely that the greatest effect of initial maze traversal is present immediately after or soon after the first run is completed. In order to find the maximal effect of a first choice upon a second, it is probable that single maze units should be used in order that the two choices may be separated by a very short time interval.

In one study such a comparison of first and later choices was accomplished by placing the rat upon a Y-unit.[2] In this situation the animal frequently brought itself back to the choice-point. Using this method, it was found that the animal tended to avoid the more recently occupied arm of the unit.

The present paper reports a continuation of the same line of investigation. It differs from the experiment just mentioned in that the experimenter, as soon as the subject has made a choice, picks it up and returns it to the original alley; where it remains until a second choice is made.

EXPERIMENT I

Experimental conditions. In this experiment the animals were placed upon one arm of a block elevated Y-unit, each arm of which was one foot high and had a running surface 2 in. wide by 6 ft. long. Except for length of arms, this unit was like that used in Part I of an earlier study.[3] The experimenter stepped behind a screen as soon as he placed the rat upon the apparatus. When the rat reached the end of one of the arms other than that upon which it had been placed, it was picked up, returned to its original position, and again permitted to progress to the end of one of the pathways. If, however, the rat did not make a choice of pathways as defined above within 10 min. after being placed upon the maze it was returned to its cage without further opportunity to explore.

Nine untrained albino rats of Wistar stock, 2 mo. of age, were used. They were deprived of food only when on the maze. Each rat was given an opportunity to complete 12 pairs of trials of the sort described above, and successive pairs of trials were separated by an interval of at least 2 days. After the fourth and eighth pairs of trials the maze was set up of new units in a new location.

Results. The ends of the two pathways were reached a total of 208 times. Of

* Accepted for publication August 6, 1934. I wish to acknowledge the assistance of Mr. E. W. Kentzman in the gathering of the data here reported.

[1] W. Dennis and R. H. Henneman, The non-random character of initial maze behavior, *Ped. Sem.*, 40, 1932, 396-405; W. Dennis, R. H. Henneman, and W. B. Varner, A further study of the initial maze behavior of rats, *ibid.*, 41, 1932, 358-368.

[2] W. Dennis and R. T. Sollenberger, Negative adaptation in the initial maze exploration of albino rats, *J. Comp. Psychol.*, 18, 1934, 197-206.

[3] *Ibid.*, 197.

these complete explorations, 53% were in the path to the animal's left as it faced the bifurcation, and 47% were in the path to the animal's right. This is approximately a chance division of choices, and is what we expected in a Y-unit on the basis of earlier studies.[4] This chance division held for both the first and the second trials of the day.

When, however, the relation between the first and second trials is examined, we find a distinct influence of the first run of a day upon the second trial of the day. There were 190 trials or 95 pairs of trials in which both first and second explorations were completed. In 82% (± 3%) of these cases the second pathway which was traversed was that part of the apparatus which was not completely explored in the first trial. In only 18% (± 3%) of the cases did both trials result in progress to the end of the same alley, whereas by chance 50% would be expected to fall in this category. Thus this experiment agrees with the previous one in showing that the effect of traversing one pathway is to decrease for a time the tendency to enter that section of the maze. The data revealed no effect of the choices of one day upon those of the following days.

Experiment II

Experimental conditions. Thus far the evidence which indicates that the exploration of a maze path decreases the likelihood of its being selected shortly thereafter has been derived from mazes in which no preference of pathways is found to exist on the first trial. The effect of traversing a path might therefore be very weak, and yet be apparent in these experiments because it would not be opposed by any other tendency. The purpose of Experiment II is to determine whether the first selection of a pathway has any influence upon the second choice when the maze is so constructed that there is a strong initial preference for a certain section of the apparatus.

We selected a block elevated T-unit and placed the animal upon one of the top arms. A previous investigation had shown that in such a situation the first choice of the animal is forward,[5] or across the top of the T in 70% of the cases. In the present instance we made use of a T each arm of which was 2 ft. long and 2 in. wide. The procedure was as in Experiment I. Twenty-two untrained albino rats of Wistar stock about 3 mo. of age were used. A pair of trials was given each animal on 3 occasions, the successive occasions being a week apart in time. Each week the apparatus was made of new blocks, set up in a new location, and the upright part of the T was changed to the opposite side of the unit in reference to the experimenter.

Results. Each rat completed each trial offered to it, so that records were obtained for 132 runs or 66 pairs of runs.

In the first runs of all days, the alley straight ahead from the point of choice was more frequently chosen. In all, this alley was the first to be followed to the end in 77% of the first runs, which is in close agreement with the findings of the experiment previously referred to. However, in the second trials, the alley which turned to the side was more often traversed to its end (58%). In other words, the effect of selecting the forward-going pathway in 77% of the first trials was to destroy this preference on the second trial. This is further shown by the fact that of the

[4] *Ibid.*, 198.
[5] Dennis, Henneman, and Varner, *op. cit.*, 362.

rats which went forward on the first trial 72% turned aside on the second trial. Combining all records, 79% of the rats took alternate pathways on the two trials, whereas the chance expectancy of unlike choices is 50%. No relationship existed, of course, between the trials of different pairs, separated by a week or more.

Summary and Discussion

The two experiments agree with one previously conducted in showing that the rat's entrance of a section of a maze unit decreases for some time its likelihood of entering the same section. Since these results were obtained with different rats, with two types of maze units and two sizes of one unit, and with two methods of inducing the animal to make a second choice, the results have at least some degree of generality.

The principle that traversal of a pathway decreases the probability of subsequent entrance, all other things being equal, is given further generality by the finding of Wingfield and Dennis that it holds true when two equally-long paths to food are offered.[6]

These studies are further evidence for the view, previously proposed,[7] that negative adaptation to pathways is an important part of maze mastery, and that a reward, instead of eliminating excess movements, prevents the occurrence of complete negative adaptation.

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[6] R. C. Wingfield and W. Dennis, The dependence of the white rat's choice of pathways upon the length of the daily trial series, *J. Comp. Psychol.*, 18, 1934, 135-148.

[7] Dennis and Sollenberger, *op. cit.*, 203.

APPARATUS

THE USE OF A CALIBRATED POTENTIOMETER IN THE MEASUREMENT OF THE GALVANIC SKIN RESPONSE

By EDWARD W. GELDREICH, University of Cincinnati

A simple and quick method for measuring the apparent resistance of the skin and the apparent resistance-change in the galvanic skin response (G.S.R.) has recently been devised and is described here. The method makes use of a potentiometer calibrated in ohms of resistance in series circuit with the *S* and a sensitive galvanometer.[1] The circuit is shown in Fig. 1. The potentiometer is a slide-wire resistor coil type connected across a single dry cell which supplies the total exosomatic potential. The galvanometer used in our laboratory is a Nadler Bros. (London) suspended coil type with an internal resistance of 590 ohms and a computed sensitivity of 2 x 10^{-8} amp. per mm. of deflection with the scale a half meter away from the mirror of the galvanometer. The scale is a standard Leeds and Northrup galvanometer scale, and the deflections are read through a telescope.

The method of calibrating the potentiometer in ohms is simple. However, before calibrating it is necessary to decide what working current is desired for use in the circuit, for it is this working current which shall be the standard current in reading all the galvanic skin responses. When the standard working current is decided upon, the calibration may be made. With the same circuit standards resistances replace the *S* in the circuit. Standard bridge boxes may be used. In our case we used ten Leeds and Northrup Wheatstone Bridge boxes, connecting the BA and GA terminals to the electrode terminals. With these ten bridge boxes in series the potentiometer was calibrated from 5000 to 100,000 ohms.

In our circuit the standard working current was about 8 x 10^{-6} amp., deflecting the galvanometer 40 cm. In order to read this deflection on a Leeds and Northrup galvanometer scale it was necessary to adjust the suspended coil so as to swing it around to read its static position at '25' (black) on the scale. Thus, a deflection of 40 cm. induced by the standard working current brought the galvanometer to read '15' (red) on the galvanometer scale. In calibrating the potentiometer and plotting each resistance, the potentiometer is adjusted so that the standard working current is read at '15' (red) on the galvanometer scale. This procedure is repeated for each resistance setting of the bridge boxes and the resistance plotted on the potentiometer scale. In determining the resistance of an *S*, the potentiometer is adjusted to read the standard working current ('15' red) on the galvanometer scale. The resistance indicated on the potentiometer scale is the apparent resistance of the *S*. Hence the apparent resistance of the *S* is compared with calibrated true resistance and the standard working current in the circuit through the *S* permits a comparison of deflection differences both for the same *S* and for other *S*s.

It will be noted the potentiometer itself is 6800 ohms, while in series with the potentiometer across the cell is an additional 6000 ohms of resistance. Thus, a

[1] Cf. W. Whatley Smith, *The Measurement of Emotion*, 1922, 170-178.

total of 12,800 ohms shunt the cell for two reasons: (1) because of the sensitivity of the galvanometer, and (2) to obtain a great enough spread for the calibration of the potentiometer. If the potentiometer is properly designed, calibration may easily be made within 100 ohms per ⅛ in. of the scale. No general design may be given here because in the final analysis the design depends upon: (1) the chosen working current in the circuit, (2) the sensitivity of the galvanometer, and (3) the available exosomatic potential. For our design we used a James G.

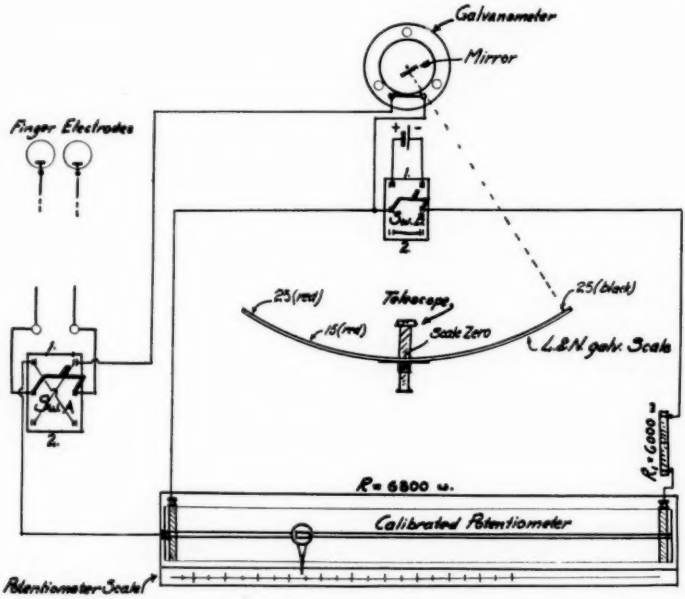

FIG. 1. POTENTIOMETER SET-UP FOR GALVANIC SKIN RESPONSE
Interpretation of scale: 25 (black) = static zero of galvanometer; 15 (red) = point on scale at which standard working current is read, the assumed zero.

Biddle (Philadelphia) coil resistor with slide contact, which has an over-all dimension of 20 in. long and 2½ in. in diam. The calibration scale is 14 in. long and permits a reading within 250 ohms of apparent resistance.

The procedure for operating this scheme is as follows: (1) The *S* is placed in the circuit, while Sw.B. is thrown in position 2, shorting out R and R_1. (2) Sw.A. is thrown to either position 1 or 2 to observe the direction of polarization. When the direction of polarization is the same as that of the exosomatic potential that position of Sw.A. is maintained. (3) Throwing Sw.B. to position 1, the potentiometer is adjusted so that the galvanometer reads '15' (red), the standard working current. The resistance read from the potentiometer scale will not be maintained until

after the so-called 'rest curve' is established. After the galvanometer has settled and the potentiometer is again adjusted to read the standard working current on the galvanometer scale, the initial apparent resistance may be read from the potentiometer scale. (4) All is now in readiness to take skin galvanic response readings. Upon stimulation of the *S* a skin galvanic response may result. The response is read in so many mm. of deflection through the telescope. Knowing the current value of each mm. of deflection, one can determine the amount of current increase represented by the response. Now a complete normal G.S.R. will deflect the galvanometer back to the assumed zero, the '15' (red) reading on the scale. Should the galvanometer not return within 10 mm. of the assumed zero, the potentiometer is again adjusted to read '15' (red) on the galvanometer scale and the initial resistance is again noted. If the galvanometer return is well within 10 mm. of the assumed zero, a floating reading may be made, *i.e.* the next G.S.R. may be read from the position where the former resided. In normal *Ss* the floating reading method may be continued for as many as ten deflections without the return deflection wandering too far away from the assumed zero. The G.S.R. may also be read in so many ohms of resistance decrease by adjusting the potentiometer to return the deflection immediately after the response attains its highest point of deflection. The point on the resistance scale of the potentiometer, upon returning the galvanometer to read the standard working current, is the amount to which the apparent resistance has decreased in the course of the response.

Important features to the advantage of this scheme are the simplicity of construction, the ease of operation, the possibility of great sensitivity and fineness of resistance calibration, and the standard working current provision for comparing G.S.R.

A DEVICE FOR STUDYING EYE-HAND COÖRDINATION WITHOUT VISUAL GUIDANCE

By O. H. Mowrer, Yale University

In the manual localization or manipulation of a seen object, three distinct stimulus-factors are ordinarily involved: (1) visual stimulation from the object; (2) visual stimulation from the hand and arm; and (3) kinaesthetic (at times also cutaneous) stimulation from the hand and arm. An analysis of the extent to which each of these three factors contributes to the development and efficient performance of eye-hand reactions is of considerable importance. It is obvious, however, that neither the second nor the third factor can be studied in isolation from the first; for with the elimination of vision of the object, reactions made to it cease to be eye-hand reactions. In *Ss* in whom the kinaesthetic (and cutaneous) sensitivity of the hand and arm is absent, either as a result of neural lesion or local anaesthetization, the first and second stimulus-factors may simultaneously be studied, independently of the third.[1] But so far as the writer is aware no entirely satisfactory method has previously been devised for eliminating the second factor (visual stimulation from

[1] K. S. Lashley, The accuracy of movement in the absence of excitation from the moving organ, *Amer. J. Physiol.*, 43, 1917, 169-194.

the hand and arm) in such a manner as to make possible the study of eye-hand reactions with only the first and third (or only the first) factor operative.[2] The apparatus here described was constructed with this end in view.[3]

When a plane mirror is inclined at an angle of 45°, the reflected image of the ceiling of a room assumes a vertical position at the same distance behind the mirror as the actual ceiling is above it. This principle of reflection has been incorporated in the apparatus shown in Fig. 1.[4] Within this apparatus is located a large mirror (X—Y) which lies in the plane mid-way between the vertical and the horizontal and which therefore divides the interior of the apparatus diagonally into an upper and lower compartment. The dimensions of the apparatus are such that when the top of the upper compartment is illuminated (by small shielded electric lamps), the reflected image of the top, as seen by *S* through the rectangular aperture in the front of the apparatus, appears in the position occupied by the (unseen) back of the lower compartment. On the inner side of the (removable) top of the upper compartment is a frame into which a piece of cardboard 10 x 10 in. may be slipped; and on the inner side of the (removable) back of the lower compartment is a second frame of the same dimensions as the first and so placed that the apparent position of the reflected image of the top frame exactly coincides with it. When a card with a centrally placed geometric figure is inserted in the top frame and a duplicate card bearing the same figure is inserted in the frame at the back of the lower compartment, the reflected image of the first figure therefore appears to *S* in the position of the duplicate (unseen) figure.[5] Since the lower compartment has no floor and no front below the level of the shelf supporting the mirror, *S*, when seated so that he can look comfortably through the opening into the upper compartment, is able to trace with a pencil on the card at the back of the apparatus the figure seen in the mirror, without seeing the moving hand or arm. When the card is removed from the frame at the back, it is a simple matter for *E* to compare the tracing with the figure previously printed on the card and in this way determine the accuracy of *S*'s performance. The task of scoring is easier still if *S*, instead of trac-

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[2] Direct vision of the hand and arm is usually eliminated in mirror-drawing experiments, but the hand and arm can always be seen in the mirror (for illustration see G. M. Whipple, *Manual of Mental and Physical Tests: Complex Processes*, 1915, 122). In the Miles two-story maze all vision of the hand and arm is effectively eliminated, but the seen (top) maze is not spatially congruent with the explored (bottom) maze. For illustration of Miles' apparatus see W. Miles, The two-story duplicate maze, *J. Exper. Psychol.*, 10, 1927, 369; also C. M. Cox, this JOURNAL, 40, 1928, facing 542.

[3] The apparatus described here was designed and constructed at the Psychological Laboratory of Princeton University during the writer's tenure of a National Research Fellowship in 1933-1934.

[4] The writer is indebted to Mr. F. V. Taylor for helpful suggestions regarding the design. The general principles involved, however, have long been known. They have previously been used by R. Dodge (*Psychol. Bull.*, 4, 1907, 10-13), by J. E. Evans (this JOURNAL, 43, 1931, 285 f.), and by J. G. Jenkins (*ibid.*, 45, 1933, 150). Jenkins ascribes the principles utilized to "a spectacular illusion of stage magic."

[5] It is essential that the figure be bilaterally symmetrical or that the figure in the top frame be the mirror image of the one in the frame at the back of the apparatus. By substituting a piece of thin plate glass for the mirror, it is possible to see the image of the figure in the top frame (due to surface reflection) and at the same time to see the figure in the frame at the back (directly by transmission). This provides a direct check upon the location of the projected image of the one figure in relation to the actual position of the other.

ing a complete figure, merely attempts to localize (with pencil or stylus) some designated part of a figure, *e.g.* the center of a bull's-eye. The nature of *S*'s task may, of course, be varied in numerous other ways, according to the demands of different experimental situations.

The apparatus here described should be found useful not only as a means of exploring and demonstrating the rôle of visual guidance in the eye-hand coördination of adults, but also as a means of studying the development of this type of response in children. It may likewise facilitate the investigation of the effect of various

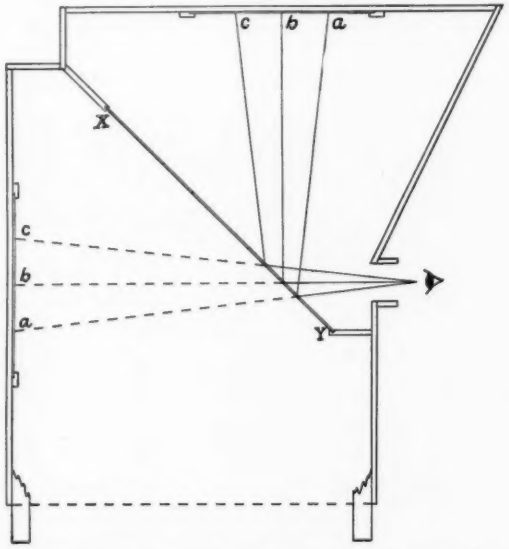

FIG. 1. DIAGRAM OF APPARATUS FOR TESTING EYE-HAND COÖRDINATION WITHOUT VISUAL GUIDANCE

The diagonal partition (X-Y) consists of a plane mirror so situated as to cause the reflected image of the top of the upper compartment, as seen by *S* through the indicated aperature, to appear in the position occupied by the back of the lower compartment. The lower compartment has no floor and the central area of the front, below the shelf supporting the mirror, is also open, so that *S*, by extending his arm, can easily touch the back of the apparatus.

positions of the eyes and head upon the tonus of the extremities. Again, it may be employed in the clinical diagnosis of certain subtle disorders of coördination (notably those arising from impaired kinaesthetic sensitivity). As an instrument for studying the effects of practice without knowledge of results its utility is obvious. Finally, by using *S*s in whom the kinaesthetic sensitivity of the hand and arm is reduced or lacking, it should be possible with this apparatus to study eye-hand coördination at its simplest level, *i.e.* with vision of the object as the only stimulus-factor; such an investigation should provide significant information regarding the fundamental character of spatio-motor integration.

A SIMPLIFIED POLYGRAPH

By HAROLD M. WILLIAMS, University of Iowa

The accompanying illustration (Fig. 1) pictures a polygraph which, while maintaining rigid standards in accuracy, approaches a minimum simplicity with a corresponding reduction in cost of construction. Its chief features are as follows.

(1) A constant speed synchronous motor is used.

(2) Paper speeds are varied by pairs of interchangeable gears, one gear mounted directly on the motor gear reducer shaft, the other gear directly on the drive roller shaft. The principle applied here is that pairs of standard gears having the total number of teeth in both gears constant will also have a constant center distance.

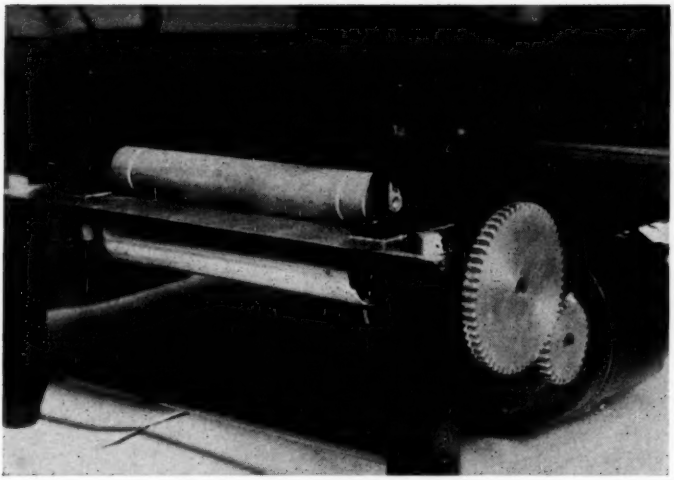

FIG. 1. A SIMPLIFIED POLYGRAPH

Each pair of gears gives two paper speeds. A number of pairs of gears of different ratio will give practically any paper velocity desired, while the motor will assure constancy of speed at any given ratio. The arrangement obviates both a variable speed motor and a third wheel in the gear train.

(3) With this arrangement it is possible to use pins in the driving roller both to assure positive control of the paper and to give a highly accurate time line. The minuteness of time divisions is proportional to the speed of the paper.

(4) The position of the roller is such that it pulls, rather than pushes, the paper past the recording pens.

(5) A heavy iron roller, riding in slots as illustrated, keeps the paper in contact with the driving roller. This auxiliary roller is sandblasted to take blotting

paper if desired. The gears may be changed in a moment, since they are locked to the shafts by means of set screws.

The total cost of the polygraph, including the motor, is but slightly over fifty dollars.

APPARATUS NOTES

Interchangeable Standards for Lifted Weights

The results of weight-discrimination studies are subject to two sources of error. The standard is used continuously throughout the experiment whereas any comparison weight is used only for one series of trials; the standard weight consequently becomes warmer than the comparison weights after several series of trials. Also, through more extended use the standard becomes smoother than the comparison weights. The discrimination is accordingly influenced by temperature and touch.

These errors may be eliminated by using two identical and interchangeable weight holders either of which may be used as the standard or as the variable. Separate weights are added to these holders to give the required differences.

The holders, for a standard of 100 grm., are constructed as follows: a base of wood 1½ x 1½ in. square, and 13/16 in. thick, is drilled at the center to hold a ½ in. wooden rod that projects 2 13/16 in. above the base. Over the rod a tube of ⅜ size of red brass 2 13/16 in. long is fitted (by heating the tubing it can be slipped easily over the wooden rod). The weight of the wood should be about 23 grm. and of the copper tube about 67 grm. After assembling the parts the combined weight should be adjusted to exactly 90 grm.

The weights to be added are made of ¾ size brass tubing and range from 10 to 24 grm., by 1-grm. differences. The approximate width of the rim of the lightest weight is ¼ in. and of the heaviest ⅝ in. A ½-grm. weight is made of a wire loop. The weights are kept conveniently in a block with recesses similar to that provided for the weights of a balance.

In using the apparatus the 10-grm. weight is placed over the copper tubing on either of the two identical holders, and another weight, say 20 grm., over the other. The standard now is 90 + 10 grm. or 100 grm. and the comparison weight 90 + 20 grm. or 110 grm. The 10-grm. weight is used throughout the experiment and the 20-grm. weight is replaced as needed by others.

Florida State College for Women. P. F. Finner

A Demonstration of Positive and Negative Visual After-Images

Students in the introductory course in psychology are often obliged to take the instructor's word for the fact that there are positive visual after-images, because a convincing lecture demonstration of them is not easy to arrange. The elementary course needs a method for arousing positive after-images in the eyes of one hundred per cent of the class in such a way that the images can be observed over a period of time.

This result has been accomplished in classes at Harvard University by the following procedure. A 500-watt projection lamp in an 8-in. conical reflector, over the front of which is fastened a plate of red glass, is used as a stimulus. The class looks

directly at the red light for about 4 sec. and then at a screen illuminated by a projection lantern. The beam from the lantern is interrupted for a second about once every second and a half, either by a motor-driven episcotister or by the hand of an assistant. Thus the students see a field which is alternately light and dark. When the field is dark, they see a vivid red image, which turns to green when the field becomes light. This alternation of positive and negative after-images persists for several minutes under favorable circumstances, allowing the students to compare the two images in various respects. The positive image is the first to fade. It loses first its color and then disappears entirely.

The effect is essentially the same as what was described by Troland[1] as the "dimming effect." In the absence of an alternating light and dark field it can be got fairly well by rapidly blinking the eyes.

The phenomenon has also been studied by students in the elementary laboratory course.

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[1] L. T. Troland, *The Principles of Psychophysiology*, 2, 1930, 95 f.

NOTES AND DISCUSSIONS

The Basis of Ligon's Theory

Ligon has published a 'three factor' theory as an explanation of why one can read the names of colors more rapidly than one can name colors[1] The data which served as the basis for the theory and also for the conclusion that all former explanations of this difference are untenable were obtained by testing 638 pupils in school grades 1 to 9 inclusive. A comparison of the results in naming colors and in reading the names of colors for the respective grades is presented in his Table I and Fig. 1.[2] In regard to these results Ligon says that "the most striking feature of these data . . . is the approximate constancy of the difference between the two functions. The curves of the functions are almost parallel. The curve of the difference is nearly a stright line, showing almost no decrease from the first to the ninth grade. This difference between the two functions is the phenomenon which we are studying, and this feaure of it will be of the utmost importance in its interpretation."[3] Ligon's means and differences for the respective grades in the two tests are given in Table I.

TABLE I

Ligon's Means and Differences Expressed in Time Units
(Number of sec. per 100 reactions)

School grade	1	2	3	4	5	6	7	8	9
Color naming	130.2	107.4	90.7	88.3	81.0	77.3	73.5	68.7	69.1
Word reading	100.6	79.6	63.2	59.6	52.1	50.5	45.0	45.3	45.8
Difference	29.6	27.8	27.5	28.7	28.9	26.8	28.5	23.4	23.3

In Table II, Ligon's time scores have been transmuted into attainment scores (the number of reactions per 100 sec.) by dividing 100 into the time score then dividing the quotient into 100.

TABLE II

Ligon's Data Expressed in Work Units
(Number of reactions per 100 sec.)

School grade	1	2	3	4	5	6	7	8	9
Color naming	76.8	93.1	110.3	113.3	123.5	129.4	136.1	145.6	144.7
Word reading	99.4	125.6	158.2	167.8	191.9	198.0	222.2	220.8	218.3
Difference	22.6	32.5	47.9	54.5	68.4	68.6	86.1	75.1	73.6

If the difference between the mean scores in the two tests is measured in absolute time units (Table I) it is found to remain 'approximately constant' throughout grades 1 to 9. If the difference is measured in absolute work untis (Table II), however, it is more than twice as great in third grade as in first grade; more than three times as great in fifth grade as in first; and nearly four times as great in

[1] E. M. Ligon, A genetic study of color naming and word reading, this Journal, 44, 1932, 103-121.
[2] *Op. cit.*, 104 f.
[3] *Ibid.*, 106.

seventh grade as in first. Thus the two procedures lead to opposite conclusions. Therefore, it is evident that both cannot be right. In fact, both are fundamentally wrong. They are based upon an unsound assumption, viz., that a difference of so many absolute units (seconds or reactions) represents an equal difference in difficulty between the tests whether the scores are numerically high or low. This would mean that the difference between scores 100 and 130 shows the same difference in difficulty as the difference between scores 50 and 80, 30 and 60, or 10 and 40 despite the fact that the difference in effort indicated in the four cases is 30%, 60%, 100%, and 300% respectively; assuming that effort is measured by the time the *Ss* invest. Peterson agrees that neither procedure furnishes a safe basis for conclusions.[4]

Since such differences cannot be satisfactorily compared when measured in absolute units, it is necessary to adopt a procedure which employs relative values. The measure of group differences, which Peterson introduced into race testing,[5] is probably as satisfactory a measure as can be employed.[6] Under this method, the measure of the group difference in the two tests is obtained by dividing the difference between the measures of central tendency (mean or median) by some meas-

TABLE III

LIGON'S DIFFERENCES EXPRESSED IN PROBABLE ERROR UNITS AND IN PERCENTAGE OF WORD READING SCORES PASSING THE MEAN OF THE COLOR NAMING SCORES
(WR = Word reading; CN = Color naming)

School grade....	1	2	3	4	5	6	7	8	9
Diff./P.E./W.R.	1.17	1.79	2.21	3.34	5.56	4.18	7.40	4.18	5.28
%WR≧CN...	78.5	88.8	93.2	98.8	100.0	99.8	100.0	99.8	100.0

ure of variability (Q, σ, or P.E.) of the group in one of the tests. Ligon's differences for grade groups 1 to 9 were divided by the probable errors of the distributions of the respective groups in the word reading test.[7] The results are shown in line 2 of Table III. As percentage of one group equaling or passing the mean of the other is a commonly used measure of group differences, these differences are also expressed in percentage of group whose score in the word reading test equals or passes the mean score of the group in the color naming test (a smaller time score) in line 3 of Table III.

Instead of showing that the difference between word reading and color naming remains "approximately constant" throughout the school grades tested, the results in Table III show that the difference between word reading and color naming increases considerably on passing up from the first grade. These results are in agreement with Lund's.[8] They are satisfactorily explained by Peterson's theory

[4] Joseph Peterson, Johnson's measurement of rate of improvement, *J. Educ. Psychol.*, 15, 1924, 271-275.

[5] J. Peterson, The use of a common unit in the measurement of race differences, *Psychol. Bull.*, 20, 1923, 424-425.

[6] J. Peterson and L. H. Lanier, Studies in comparative abilities of whites and negroes, *Ment. Meas. Monog.*, 1929, (no. 5), 44.

[7] As the probable errors of Ligon's distributions were not given they were calculated from the probable errors of the means.

[8] F. H. Lund, The rôle of practice in the speed of association, *J. Exper. Psychol.*, 10, 1927, 424-433.

that, "one particular response habit has become associated with each word while in the case of colors themselves a variety of response tendencies have developed."[9] This is supported by experimental results by Stroop,[10] which show by an interference method that the associations between the words and the reading responses are much more effective than those between the colors and the naming responses. Since college students, who certainly have well formed associations between the five common colors and their color names, were used as subjects, it is reasonable to conclude that other responses such as admiring, reaching for, avoiding, grasping, etc., interfered with the requested response of naming colors. As children pass from the first grade upward the reading response to words is becoming habitual thus decreasing the time for reading words. During the same period, a variety of response tendencies to color stimuli are being developed which interfere with the naming response and tend to increase the time for naming colors. These two changing conditions somewhat modified by the organic development of the children and the similarity of the tests account for the increase in the difference between the means.

TABLE IV

LIGON'S CORRELATIONS, RATIOS AND RANK SQUARE DIFFERENCE CORRELATIONS

Correlations

School grade	1	2	3	4	5	6	7	8	9
r (CN & WR)	.709	.558	.162	.594	.405	.506	.275	.394	.423
r (CN & Diff.)	.500	.460	.663	.644	.944	.763	.855	.746	.893
r (WR & Diff.)	−.220	−.400	−.599	−.284	−.122	−.062	−.249	−.323	−.222

Ratios

School grade	1	2	3	4	5	6	7	8	9
σCN/σWR	1.08	1.00	1.09	1.14	1.90	1.73	1.94	1.38	1.64

Rank square difference correlations

Series 1	Series 2	Rho	Series 1	Series 2	Rho
r (CN & Diff.)	r (WR & Diff.)	−.49	ratio of σs	r (CN & Diff.)	+.90
r (CN & WR)	r (CN & Diff.)	−.56	ratio of σs	r (WR & Diff.)	−.55
r (CN & WR)	r (WR & Diff.)	−.33	σ CN	r (CN & WR)	+.44
			σ WR	r (CN & WR)	+.47

In his Tables III and IV,[11] Ligon gives the data shown below in Table IV. He interprets the correlations between the tests as showing that color naming and word reading are related and yet are independent in 'some factors' at least. He regards the correlations of the tests with the difference, the ratios, and rank square difference correlations as further evidence of the presence of independent factors. These also serve as a basis for determining the relationship between the factors. He accounts for the relationship between the tests by the presence of a common factor and the independence of the tests by the presence of special 'color naming' and 'word reading' factors.

[9] J. Peterson, L. H. Lanier and H. M. Walker, Comparisons of white and negro children, *J. Comp. Psychol.*, 5, 1925, 281.

[10] J. R. Stroop, Studies of interference in serial verbal reactions, *J. Exper. Psychol.*, 18, 1935, (in press).

[11] Ligon, *op. cit.*, 108 f.

The critical examination of the data given in Table IV will be preceded by a report of the results of a similar experiment of our own. The correlations, ratios, and rank square difference correlations that were calculated from data obtained by testing 31 college students who had practiced 8 days in a color naming test are given in Table V. Different forms of a new type of color test were used.[12] The colors to be named appeared in the print of names of other colors—red ink was used to print the words blue, green, brown, and purple; blue ink was used to print the words red, green, brown, and purple; etc. The first trial in the test was

TABLE V

CORRELATIONS, RATIOS, AND RANK SQUARE DIFFERENCE CORRELATIONS CALCULATED FROM DATA FOR EIGHT TRIALS IN A COLOR NAMING TEST

Correlations

Between trial 1 and each succeeding trial

Trials	1 & 2	1 & 3	1 & 4	1 & 5	1 & 6	1 & 7	1 & 8
r	.829	.779	.649	.658	.540	.754	.574

Between trial 1 and trial 1 minus each succeeding trial

Trials	1 & 1−2	1 & 1−3	1 & 1−4	1 & 1−5	1 & 1−6	1 & 1−7	1 & 1−8
r	.325	.538	.509	.497	.674	.673	.643

Between trials 2, 3, 4, 5, 6, 7, 8 and trial 1 minus each succeeding trial

Trials	2 & 1−2	3 & 1−3	4 & 1−4	5 & 1−5	6 & 1−6	7 & 1−7	8 & 1−8
r	−.295	−.095	−.368	−.329	−.240	−.136	−.218

Ratios of σs 1 to that of each succeeding trial

Trials	1 & 2	1 & 3	1 & 4	1 & 5	1 & 6	1 & 7	1 & 8
Ratios	1.09	1.26	1.17	1.08	1.41	1.32	1.27

Rank square difference correlations

Series 1	Series 2	Rho	Series 1	Series	Rho
rs row 2	rs row 3	−.54	Ratio of σs	rs row 2	+.89
rs row 1	rs row 2	−.75	Ratio of σs	rs row 3	−.57
rs row 1	rs row 3	−.07	σs last 7 trials	rs row 1 last 7 tri.	+.68

correlated with each succeeding trial to obtain the coefficients in row 1. The first trial was correlated with the difference between it and each succeeding trial giving the coefficients in row 2. The results of correlating the second and each succeeding trial with the difference between itself and trial 1 are shown in row 3. Thus row 1 shows the correlations between pairs of tests (trial 1 being in each pair); row 2 contains correlations of one of each pair with the difference between the two members of the pair; and row 3 gives the correlations of the other test of each pair with the difference. Row 4 shows the ratios of the standard deviation of trial 1 to that of each succeeding trial. The bottom section of the table presents the rank square difference correlations between correlations, ratios, and standard deviations as indicated.

In Tables IV and V the same statistical procedures have been employed. There is a striking resemblance between the data of the two tables. In each table the difference correlates positively with the test of the pair having the larger score and

[12] Stroop, *op. cit.*

negatively with the test having the smaller score. The ratio of the standard deviations increases as the correlation between the difference and the test with the larger score increases. The rank square difference correlations agree in sign and tend to agree in magnitude. In fact, the only difference that might be significant is found in row 1. The coefficients between the tests in Table IV (Ligon's data) are lower and more variable than those in Table V. These will be considered later.

It is evident that the latter results cannot be explained by special color naming and special word reading factors as only color naming tests were used. Since the correlations with the difference, the ratios of the standard deviations, and the rank square difference correlations in my results are comparable to those obtained by Ligon when color naming tests and word reading tests were both used, it is reasonable to infer that the special factors of color naming and word reading do not explain Ligon's results. Neither do his results furnish evidence of the presence of these special factors.

Referring to the coefficients given in row 1 of Table IV, Ligon says, "Yet the variability of the coefficient and the low level to which it drops, also, indicates that there are some factors at least in which the two functions vary independently."[13] The only factors designated are the special factors of color naming and word reading which Ligon declares to be "organic or structural." Structural causes should have a tendency toward constancy. How can these special "organic or structural factors" account for the extremely low coefficients in Grades 3 and 7, and the much higher coefficients in Grades 4, 6, and 9? (Table IV, row 1.) At what stage in the individual's development do these "factors" appear, mature, and wane? In fact, the variability of these coefficients rather indicates the absence of any such organic factors. Why should the low level to which the coefficient drops indicate the presence of special organic factors when the variability of the same coefficient indicates their absence? The decrease in the coefficient shows that the relationship between the tests is closer in the earlier grades than in the later grades. This should be expected as the reaction in the word test has become more habitual in the later grades and that in the color test has developed more interference. A test in simple or practiced reactions and one which presents a more complex or novel situation do not generally correlate very highly.

In Ligon's study of errors,[14] the average number of errors for the respective grade groups in order of grade is as follows: 1.71, 1.73, 1.70, 1.40, 1.59, 1.81, 1.41, 1.25, and 1.13 in the color naming test; and 1.36, 0.49, 0.61, 0.59, 0.42, 0.49, 0.35, 0.52, and 0.46 in the word reading test. Only in Grade 1 is the mean error in the word reading test comparable to the mean error in the color naming test. In all other grades it is much lower. This shows the presence of some disturbing element in the color naming test for all grade groups which is not present in the word reading test except for Grade 1. The tendency to make errors in the color naming test is fairly constant throughout the grades used. This is in agreement with the theory that other response tendencies interfere with naming colors.

In his discussion, Ligon makes the following statements: "The 'practice hypothesis' assumes the independence of the two functions. The correlations found between them at the various grade levels averaged 0.450. These two facts seem to me to be quite complete proof that the 'practice hypothesis' is untenable."[15] Why should

[13] Ligon, *op. cit.*, 108. [14] *Ibid.*, 114. [15] *Ibid.*, 119.

the "practice hypothesis" necessarily assume the independence of color naming and word reading? Is it necessary that two activities be absolutely independent in order that practice in one does not show an equal improvement in the other? If this were true the transfer effects of training would always be either 100% or zero. Experimental results in transfer studies do not agree with such a statement.

Conclusions. (1) Ligon's difference between color naming and word reading really does not remain approximately constant for school grades 1 to 9 inclusive but increases considerably on passing upward from first grade. Thus Ligon's results agree with previous findings.

(2) The correlations of color naming and word reading with the difference, the ratios of the standard deviations of the tests, and the rank square difference correlations presented by Ligon are not due to 'special color naming and word reading factors' but to the mathematical relationships that pertain when a series of pairs of tests yielding time scores show a general decrease in the relationship between the pairs as the series progresses.

It seems reasonable that this general conclusion might be deduced in the form of a practical suggestion. *Extreme caution should always be exercised in interpreting data which have been treated statistically, to see that the results of statistical manipulation are not mistaken for experimental results.*[16]

Peabody College J. Ridley Stroop

The Accuracy of the Constant Method

Satisfactory experimentation in the field of psychophysics is somewhat hindered by the lack of adequate formulas for estimating the sampling errors of the procedures involved. A number of investigators have attempted to supply the necessary formulas when the experiment follows the methodology characterized by the term 'constant method.' Urban,[1] in 1909, presented a formula for the probable error of the interval of uncertainty. Since that time a number of writers have criticized Urban's results and have shown that for specific cases his formula gives an estimate of the probable error quite inconsistent with what would be expected upon examination of the data.

Thomson was the first to point out that the Urban formula gave unacceptable results and developed a formula for the probable error of the limen.[2] While no attack has been made on the theoretical correctness of Thomson's work, his resulting equation is far too complicated to be used extensively. Even with acquaintance with the formula, three or four hours are required for computing a single probable error by one accustomed to the use of rapid calculating machines. Later Boring proposed a simpler formula,[3] based on the analogy between the threshold computed by the constant method and the mean of an ordinary frequency distribution. Culler claimed that the threshold was more analogous to the median of a frequency distribu-

[16] The writer expresses his appreciation to Dr. Joseph Peterson for valuable suggestions and criticisms of this study.

[1] F. M. Urban, Die psychophysischen Massmethoden als Grundlagen empirischer Messungen, *Arch. f. d. ges. Psychol.*, 15, 1909, 261-355; 16, 1909, 168-227.

[2] G. H. Thomson, The accuracy of the $\varphi(\gamma)$-hypothesis, *Brit. J. Psychol.*, 7, 1914, 44-55.

[3] E. G. Boring, The number of observations on which a limen may be based, this Journal, 27, 1916, 315-319.

tion and that its sampling variation was the same as that of a median[4] He also disagreed with the method which Boring used to determine the value that should be inserted in the formula to represent the number of cases. Culler first suggests that the number of cases be equated with the number of series, but later proposes another formula where the number of series is weighted.[5]

The experimental psychologist is therefore confronted with a number of conflicting formulas and no criterion to indicate the correct one. It has been difficult to compare the formulas empirically since there exists no sufficiently large or homogeneous set of data. Data collected on different individuals are not suitable since the variation between the obtained thresholds is necessarily greater than the sampling variation which the formulas propose to measure. Even data collected on a single individual, under uniform conditions in the same laboratory, are unsuitable because of unavoidable changes in the psychological and physiological organization of the subject from day to day. Attempts have been made to get a number of comparable sets of data by a sub-division of the observations on a single subject. Obviously only a few sets can be obtained in this way and the resulting estimate of the variation of the thresholds is inadequate.

It is the object of this paper to present the results of the analysis of a large group of experimental, but psychologically fictitious, data which have been collected under conditions against which the above objections cannot be made. Comparisons will be made between the sampling variation of the obtained thresholds and the estimates of that sampling variation given by the various formulas. Thomson, Boring, and Culler give formulas for estimating the sampling variation of the threshold or limen. Urban, however, specifically limits the applicability of his results to the interval of uncertainty.[6] Inasmuch as the present material was collected under conditions comparable to those in which no 'equal' judgments are permitted, the interval of uncertainty is in every case equal to zero. For this reason the data will be used only to compare those formulas which estimate the sampling variation of the threshold.

Method of collecting the data. We define x_1 as a certain stimulus value and assume that there is a fixed probability p_1 that x_1 will cause the report of a perception. In addition we assume that as x increases from x_1 to x_n the associated probabilities will also increase. If the functional relation between x and p is continuous in the mathematical sense, it is impossible to select any one value of x so that its presentation always elicits a report of a perception, but every value of less magnitude never elicits such a report. It seems that, due to the psychological and physiological flux of S, the threshold is not constant but varies from instant to instant. In this way the problem of determining the threshold becomes a statistical problem of estimating the central tendency of a frequency distribution. Actually we are not able to obtain the frequency distribution that we wish to consider. If any stimulus value x_1 is presented to S he will report a perception not only when his threshold is at the value x_1 but also when his threshold has any value less than x_1. For this

[4] E. Culler, Studies in psychometric theory, *Psychol. Monog.*, 35, 1926, (no. 163), 85-90.

[5] E. Culler, Studies in psychometric theory, *J. Exper. Psychol.*, 10, 1927, 463-477.

[6] F. M. Urban. The accuracy of the method of constant stimuli, this JOURNAL, 38, 1927, 252-256.

reason psychophysical data are not represented by frequency curves but by integral curves of frequency curves.

As a basic hypothesis we take the $\varphi(\gamma)$-hypothesis which assumes that the proper integral curve is the integral of the normal probability curve. This is the fundamental hypothesis in the treatment of data according to the constant method. For arbitrarily selected constants we can find the value of p associated with any value

TABLE I

THEORETICAL STIMULUS VALUES AND THE ASSOCIATED PROBABILITIES

x-values	−2.00	−1.20	−0.40	+0.40	+1.20	+2.00
probability	.0228	.1151	.3446	.6554	.8849	.9772

of x by reference to a table of the normal probability integral. For convenience we shall use the curve with zero mean and unit standard deviation.

We approximate a laboratory experiment by selecting certain values of the stimulus x. Following usual laboratory procedure we can select six such values and select them at equal intervals along the scale. Table I gives the values selected and the associated theoretical probabilities.

Next we wish to obtain a number of approximations to the probabilities in Table I in a manner which is free from the psychological variation found in laboratory data. To do this we utilize Tippett's *Random Sampling Numbers*.[7] Tippett's numbers consist of a random selection of 40,000 digits combined to give 10,000 four-digit numbers. The numbers therefore range from 0000 to 9999. If we wish, for example, to draw a sample of 50 from a population in which the probability of getting a certain result is 0.5000, we can examine any random selection of 50 numbers and see how many of them are less than 4999. It would seem that we should tabulate the number equal to, or less than 5000, but this would be the case if the lowest possible number were 0001 and not 0000. From Table I it is seen that the probability of getting a specified result when the stimulus -2.00 is presented is 0.0228. We get an approximation to this probability, based on 50 observations, by examining the first column of 50 members and noting how many of them are

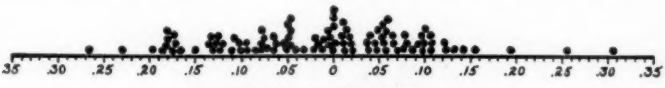

FIG. 1. DOT-DIAGRAM SHOWING DISTRIBUTION OF THE 100 THRESHOLDS. The parameters of this distribution are given in Table II.

equal to, or less than, 0227. Approximations to the probabilities associated with the other stimulus-values are obtained in a similar way. Any set of approximations consisting of each stimulus-value with its empirical relative frequency is analogous to the results of the observations made in a laboratory upon an *S*. We have avoided, however, all the confusing elements that might occur in laboratory experimentation, and have introduced only variation of a random sampling nature.

[7] L. H. C. Tippett, *Random Sampling Numbers: Tracts for computers*, 1927, viii, xxvi.

By the method explained, 100 sets of data, each set consisting of 50 observations of each of the six stimulus-values, have been collected. From each set the threshold has been computed by the procedure outlined by Brown and Thomson.[8] Inasmuch as these 100 thresholds are drawn from a known and constant population, they are ideal for comparing various probable error formulas.[9]

Results. Fig. 1. gives a frequency dot-distribution of the 100 obtained thresholds. The theoretical value is zero and it is seen that the obtained values are scattered

TABLE II

PARAMETERS OF THE DISTRIBUTION OF OBTAINED THRESHOLDS

Minimum value	Maximum value	Range	Mean and standard error	σ and standard error
−.2660	+.3049	.5709	−.0106 ± .0106	.1058 ± .0075

around this in a random manner. The parameters of the distribution shown in the figure are given in Table II.

The value of 0.1058 ± .0075 is therefore an approximation of the sampling variation of the threshold computed by the constant method under the conditions of experimentation used here. It is this value that the sampling error formulas propose to estimate. In Table III we compare this value with the estimates given by the various formulas. Most formulas estimate the *probable error* rather than the *standard error* and consequently the value given above is multiplied by the factor .6745.

TABLE III

COMPARISON OF OBTAINED PROBABLE ERROR WITH ESTIMATES BY FORMULAS

	Obtained value	Value by formula	Δ	Δ/P.E. obt.
Boring	.0714 ± .0051	.0513	.0201	3.94
Culler (1st.)	.0714 ± .0051	.1196	.0482	9.45
Culler (2nd.)	.0714 ± .0051	.0909	.0195	3.82
Thomson	.0714 ± .0051	.0643	.0071	1.39

In the first column the reference to the formula is given. The second column gives the sampling variation of the obtained experimental thresholds, and the third column gives the estimation of the sampling variation from the formula. In computing the values by the formulas we take advantage of the fact that we know the constants of the population. We enter the formula with these theoretical values rather than those taken from any sample. The validity of the result is thereby increased. The next column gives the difference between these two values and the fourth column gives the difference divided by the probable error of the obtained

[8] W. Brown and G. H. Thomson, *Essentials of Mental Measurement,* 3d ed., 1925, 64-71.

[9] A more complete explanation of the method of collecting the data and tests showing that they are truly random is given in a previous article, cf. F. E. Linder, A statistical comparison of psychophysical methods, *Psychol. Monog.,* 44, 1933, (no. 199), 1-20.

value. In any case where this ratio exceeds the value 3.00 we may begin to suspect that the two estimates are essentially different.[10]

By this criterion we see that only the formula proposed by Thomson gives an estimate that differs from the experimental value by an amount that might reasonably be ascribed to sampling error. The other formulas seem to give values inconsistent with the experimental results of this study. The formula given by Boring is based on the analogy between the threshold computed by the constant method and the mean of the conventional frequency distribution. Those of Culler are based on the analogy between the limen and the median of a frequency distribution. While it is true that the limen and the median are similar in interpretation, yet the method of computation is quite different and there is no a priori reason for thinking that the two procedures will have the same sampling efficiency. When we have fitted the normal integral curve prescribed by the constant method, the computed threshold is numerically identical not only with the median of the differential curve but also with its mean and mode. From this it might be argued that the sampling variation of the threshold could be estimated by the formula for the probable error of either mean, median, or mode. As a matter of fact, if we compute the probable error by the regular formula for the probable error of the mean (using Culler's method for finding the number of cases) we get an estimate much nearer the obtained value than that given by any of the other formulas (0.0725). This, however, is insufficient justification for its general application and the experimental psychologist could have no assurance that he would not be led into error.

Summary. An experimental method is presented by which sampling problems in psychophysical methodology may be investigated. On the basis of data collected by this method, comparisons are made between the sampling variation of thresholds computed by the constant method and estimates of the sampling variation given by various probable error formulas. With the exception of the formula proposed by Thomson, no probable error formula for the constant method seems to give trustworthy results. Since Thomson's formula requires a prohibitive amount of computation, the experimental psychologist is left without any satisfactory measure of the sampling error of his procedures.

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FORREST E. LINDER

SOME AMBIGUOUS CONCEPTS IN THE FIELD OF "MENTAL ORGANIZATION"

Investigations of trait relationship, or "mental organization," beginning with the pioneer work of Spearman,[1] have at present attained very considerable proportions both in the extent and variety of subjects and test material employed, and in the development and refinement of mathematical techniques for the treatment of the

[10] The value of the ratio that shall be taken to indicate a significant difference is, of course, somewhat arbitrary. Certain modern statisticians (*e.g.* R. A. Fisher, *Statistical Methods for Research Workers*) begin to suspect a true difference when the ratio is such that the probability of significance is less than 0.05. A more rigid criterion is to take as significant a ratio that corresponds to a probability of 0.02. By this criterion any ratio greater than 3.45 indicates a significant difference.

[1] Carl Spearman, "General intelligence" objectively determined and measured, this JOURNAL, 15, 1904, 201-293.

data. The problem of trait relationship is coming to the attention of an increasingly large body of psychologists, and its implications for a variety of other problems are being rapidly recognized. It seems timely, therefore, to clarify certain concepts and points of terminology which have caused some confusion. Among workers in the field of mental organization, there is gradually developing a tendency towards mutual agreement and consistency of interpretation as more inclusive theories and viewpoints are beginning to replace the necessarily limited earlier theories. Recent studies[2] suggest more and more, for example, the influence upon the relationship obtained of such conditions as age, developmental status, and educational background of the subjects, relative homogeneity of the group, and specific materials employed in the tests. It would indeed seem unwarranted to expect any one rigid formula to fit all results, or to insist that the above conditions be "ruled out" by arbitrarily selecting any one age, educational level, degree of heterogeneity, or type of material in order to find "true" mental organization. The very differences in the findings of various investigators are illuminating when considered in relation to the factors which brought them about, since these factors are thereby shown to be of fundamental significance in determining and modifying mental organization.

With differences among current theories of mental organization, the present paper has no concern. These differences are gradually working themselves out with the accumulation of more data and the more comprehensive interpretation of such data. There still remain, however, a number of common criticisms repeatedly brought to the fore by that larger body of psychologists who, although not immediately concerned with statistical techniques in their own research, nevertheless exhibit various degrees of interest, concern, or skepticism over the psychological implications of studies which do employ such techniques.

The use of the term "mental organization" to denote the field of investigation creates a rather unfortunate impression because of its historical connotations. The term is, it seems, strongly suggestive of a reversion to early scholasticism and faculty psychology. It is the belief of the writer, however, that such a reversion is by no means implied. The major differences between faculty psychology and the current theories of mental organization will be summarized briefly. Maher, representing the scholastic viewpoint, writes, "By a faculty is meant the mind's capability of undergoing a particular kind of activity; thus our sensations of colour are due to the faculty of vision, our judgments to the faculty of intellect, and our volitions to the faculty of will."[3] The recent statistical studies are concerned not with vague, mystical, psychical entities, but with *concrete behavior manifestations*. The data of such studies are the varied responses of individuals to standardized test situations or stimuli, and it is only with the relationships among groups of such concrete behavior manifestations that the current theories are concerned. By relationship is meant nothing more than the tendency for a given response to vary concurrently with some responses and independently of other responses. This tendency for concomitant variation among a group of responses, or behavior manifestations, is all that a *group factor* can mean statistically. If some

[2] Cf. Anne Anastasi, Further studies on the memory factor, *Arch. Psychol.*, 22, 1932, (no. 142), 1-60; H. E. Garrett, R. Bryan and R. E. Perl, The age factor in mental organization, *ibid.*, 26, 1935, (no. 176), 1-31.

[3] Michael Maher, *Psychology: Empirical and Rational*, 1929, 29.

investigators have gone further and posited entities underlying the group factors, such entities are quite independent of the statistically established group factors and are not implied by them. This suggests a second difference between scholastic faculties and statistical factors; namely, that the latter are not in any sense *causal.* Since the group factor is nothing more than a condensed statement of the fact that certain responses do vary concomitantly among themselves and independently of other responses, it certainly cannot be offered as an explanation or cause of such a relationship. Finally, as recent experiments have especially demonstrated, statistical factors are not fixed, permanent, immutable, or innate.[4] They are statements of relationships found under given conditions at a given time. The study of the conditions which bring about changes in such relationships are an even more fruitful approach to the problem than the identification of the factors themselves.

Even if the purely statistical definition of "factor" is accepted, however, there are still heard dissenting voices which object that it is not clear whether the factors are in the tests, in the group, or in the individuals. There can be only one answer to this: the factor is a mathematical statement of relationships within the *individual's responses to the test stimuli.* If the tests are altered in any way, then naturally the factor will be altered, since the individual is responding to different stimuli. The rôle played by the group needs further clarification. We are essentially concerned with the relationship of behavior within the individual, so that we shall ultimately be enabled to say that if the individual is 'proficient' in response *A,* he will also be 'proficient' in response *B,* but not necessarily so in response *C.* Thus we have a statement of what a group factor means in terms of the individual. The difficulty arises in defining the term "proficient," or differently put, in finding a standard, a point of reference, or measuring rod whereby the individual's reaction may be unambiguously described. The group is required in order to furnish such a point of reference. We can now restate our hypothetical group factor. The individual who is more 'proficient' than the average of a given group in response *A* will be more 'proficient' than the average of the same group in response *B,* but not necessarily so in response *C.*

Objections will immediately be raised to such an oversimplified statement. It will be argued that the relationship of *A, B,* and *C* as described above does not hold for *every* individual in the group, but only for the majority. This, of course, is only another way of saying that the correlation between *A* and *B* is not perfect. This correlation is lowered by the fact that any response made by the individual—especially by such a complex organism as the human—is complex, however physically simple the stimulus may be. Since more than one elementary type of response is occurring simultaneously, any one individual's standing in *A* or *B* in the group may be displaced because of the influence of other simultaneous responses, *x, y, z.* The latter are "specific factors" in the terminology of mental organization.

A second objection may be raised that the individual's 'factors' do not remain constant when he is tested in different groups. It will be seen that this must necessarily follow from the above arguments. If the individual's behavior is described in terms of the group as a point of reference, it will be differently described depending upon the group. It is a fact of common observation that an individual who is above average in elementary school, becomes average in high school, and

[4] Cf. Garrett et al., *op. cit.*

may fall behind and be distinctly inferior if he goes to college, not because of any deterioration in the individual, but because he is evaluating his performance in terms of an ever-rising standard. Similarly, we should expect the relationships among various types of behavior to differ when the behavior is measured in terms of the standards of different groups. The individual may be above the average of one group in responses *A, B, C,* and *D,* whereas in another more select group, he may be above the average in only *A* and *B.* It follows from this discussion that findings on mental organization should always be described with reference to the particular type of group in which they are to be applied.

The attempt has been made in the present paper to give the most natural, objective, and simple interpretation to some widely misunderstood and variously interpreted terms. It has been assumed that these interpretations are essentially those implied whenever such terms are employed in statistical studies on mental organization. Should these be at variance with the interpretations intended by any workers in the field, this difference should certainly be brought to light. The discovery of such differences, if there be any, may well be considered an incidental purpose of the present paper.

Columbia University ANNE ANASTASI

THE ISHIHARA TEST FOR COLOR-BLINDNESS: A POINT IN ETHICS

In May, 1935, there appeared in the weekly supplement of an American newspaper, under the caption "Are you color-blind?", reproductions in color of 14 of the 16 plates of the *Ishihara Test for Colour-Blindness.* Under each plate was printed the normal reading, along with that for one or more of the commoner forms of color deviation. Thus, overnight, the test fell from the level of a scientific instrument—invaluable in preliminary diagnosis in the medical office, in universities, in the army, in the navy, and in aviation—to the status of a parlor diversion, on a par with the cross-word puzzle, or "Ask me another."

At the date in question, the writer was accumulating data from unselected populations in North Carolina and in the vicinity of Ithaca, N.Y., on certain of the variable Ishihara responses (notably to Plates 4, 5, 10, 11, 12, and 13). These, it was apparent, might correlate less with red- and green-receptor sensitivity than with certain racial or environmental variants of color vision, such as shortening or lengthening of the spectrum at the red or violet ends. A scheme was therefore under way to check such responses against the results in special tests of a more detailed and scientific nature. Like many other psychologists who have utilized the Ishihara plates, the writer was proceeding on the assumption (unfortunately ill-grounded) that the test was properly copyrighted, and that copies were being dispensed by the American and European agents to accredited psychologists and physicians only. Certain control groups at Ithaca were about to be tested, when the reproduction of the Ishihara plates was discovered in their hands.

The publishers of the colored supplement maintain that they were within their legal rights, since they obtained their copies through legitimate channels, and these copies carried no indication of a copyright. Up to this time, it may be noted, the integrity of the test had been fairly well sustained by the ethics of the press and of the profession: only single sample plates or simulations of them designed to demonstrate the principle having appeared in print. A psychologist and an ophthalmologist,

it is true, had been guilty of needlessly inserting the key in their research statistics, though in technical periodicals accessible only to few.

The original of the test, as every psychologist knows, is a clever adaptation by Dr. Shinobu Ishihara, professor of ophthalmology in the Imperial University of Tokio, of a principle elaborated much earlier (in 1876) by Dr. Stilling of the University of Strassburg.[1] Digits made up of colored spots of varying size and chroma (in order to trap different grades of color weakness) are outlined against a background of spots in which hue as well as size and chroma may vary. In many of the plates use is made of the 'confusion colors' of the 'red-green blind' (such as yellow-green and orange-red). With these are interwoven the two 'primaries' which ordinarily lose all chroma in the commoner forms of color deviation (vertigris and crimson or magenta). This is contrived by Ishihara in such fashion that the design, through color contrast, may present one digit to the normal eye, and quite another to the color deviate, through the blending of a portion of the outline with the ground. Elimination of the red and green valences of a mixed or a pure color, in short, may either obliterate a digit, or transform and integrate it into another, outlined in blue or brown or gray against a different ground. Dr. Ishihara makes provision also, as did his predecessor, Stilling, for detection of shortening of the spectrum in the long-wave region (scoterythrous vision). Though plates for diagnosing the rarer blue-yellow defect such as Stilling supplies are lacking, many prefer the Ishihara version for its simplicity, compactness, reversibility of plate order, shortening of the test distance, and the subtlety of the duplex digit feature noted above. The fifth edition (dated 1925) bears the names of E. Koshiba, printer, and T. Kanchara, publisher, Tokyo, Japan.

The caption carried by the colored supplement copy *Are you color-blind? This will tell you,* proves to be quite fallacious. A three-color process is used in the reproductions, but the printing is overhasty, and successive inkings seldom fall true. Hence the overlapping blue or yellow edges of composite colors enable *Os* who customarily make the 12 typical errors to lower their score to 6 or fewer, when the key printed immediately below each plate is covered. When the key is exposed, their errors fall still lower. Many red-green blind able to make a fair showing with this copy will now maintain their cases have been unfairly diagnosed by the clinician. Omission in the copy of any caution as to the distance for which the test was designed (30 in.) will inevitably falsify the results further.

The explanatory text following the plates is equally misleading. The English of Ishihara's *Introduction* has itself long stood in urgent need of revision, having misled more than one psychologist of standing with its complicated subtypes, its obsolete "complete and incomplete red-blindness and green-blindness," and its use of the term *bright* for intensity or saturation. The journalistic version is one degree worse. The public is informed that "partial color-blindness is the result of deficiency in the red-green sense and *an accompanying weakness in the blue-yellow sense* (italics ours); and that in 'green-blindness' (purely a logical homologue of scoterythrous vision, unverifiable in the authentic literature) "the green part of the spectrum

[1] J. Stilling, *Pseudo-Isochromatische Tafeln für die Prüfung des Farbessinnes.* In 1926, a seventeenth edition, with new plates, was brought out by E. Hertel. The English directions for the use of the plates in diagnosis is, however, inadequate and misleading.

shows no color" (an excerpt from Ishihara's badly Englished *Introduction*). Actually, of course, most greens are decidedly colorful to the red-green color-blind, for whom the blue and yellow valences of stimuli ranging from about 490 to 560 mμ do duty for the missing green one. Except for very low or very high intensities, only a very narrow strip around 500 mμ seems to go gray in the general run of cases.

Fortunately, though the paper printing the spurious test boasts 'the greatest circulation in the world,' the stock is flimsy, the million copies estimated to have been placed in circulation are probably today largely reduced to pulp. It might be argued therefore that in calling the attention of an unsophisticated public to a widely prevalent and little recognized defect, carrying the weightiest vocational implications, the printing has served a useful end. The rub lies in the fact that an easily memorized key is now available to the hundreds who have long sought it. With it they may secure access to occupations (such as that of the engineer) in which the lives of thousands are jeopardized by an erroneous interpretation of color-signals. True, the clinician may vary the order of the test plates, to the momentary confusion of the would-be dissimulator. But even if all the users of the Ishihara test (experimentalists included) could be warned, there are certain critical plates which, in whatever order given, could now probably be identified by students of the colored supplement copy.

The present edition must be replaced by a new one. In this there should be more two-digit plates, to confuse the defrauder; also a more logical distribution of rectilinear and curvilinear digits, since these are not coördinate in legibility. The new version should be copyrighted, and its sale to professionals only guaranteed. Meanwhile the mischief done to science and to the general public by the commercial exploitation of the present edition is irreparable.

Cornell University ELSIE MURRAY

THE THIRTIETH ANNUAL MEETING OF THE SOUTHERN SOCIETY FOR PHILOSOPHY AND PSYCHOLOGY

The thirtieth annual meeting of the Southern Society for Philosophy and Psychology was held in the Noel Hotel, Nashville, Tenn., under the auspices of Vanderbilt University and George Peabody College, April 19 and 20, 1935. The first session of Section A, Psychology, beginning Friday at 9 A.M. was under the chairmanship of the President, J. E. Winter (West Virginia). The following papers were read: Constructive Activity as a Process in Learning, J. J. Ray (Peabody); Consonance Discrimination in Trained and Untrained Subjects, Eugene Bugg (Vanderbilt); Practice and Fatigue Effects on Visual Readjustments, Z. J. Schoen (Virginia); Does Practice with Inverting Lenses Make Vision Normal? Joseph Peterson (Peabody); The Relationship between the Character of the Transfer and Retroactive Inhibition, M. E. Bunch (Washington, St. Louis); Analysis of Learning by Rational Methods, Buford Johnson (Johns Hopkins); Some Recent Experiments in Memory and Their Bearing on Theories of the Nature of Brain Traces, H. M. Johnson (American).

In Section B, Philosophy, with H. C. Sanborn in the chair, the papers were: Implication and Deducibility, Joseph Burk (Kentucky); Modality and Probability, L. O. Katsoff (Virginia); Modal Functions, John Kuiper (Kentucky); The Presuppositions of the Galilean Mechanics, W. S. Weedon (Virginia); The Electrical Syllogism, E. E. Richardson (George Washington).

In Section A, Friday afternoon, with R. M. Dorcus (Johns Hopkins) serving as chairman, the papers were: The Sensitivity of the Skin to Mechanical Vibration, B. von Haller Gilmer (King, Bristol, Tenn.); A Critical Test of Binocular Interaction, S. R. Wallace, Jr. (Virginia); An Evaluation of Current Theories of Contrast, F. A. Geldard (Virginia); Experimental Study of Color Naming, J. R. Stroop (Tennessee Polytechnic Institute); An Illusion Due to Occultation, F. C. Paschal (Vanderbilt); The Effect of Fatigue or Boredom on Teachers' Marks, E. S. Dexter (Agnes Scott); Is There a "Unit Trait" of Volitional Perseveration? R. M. Dorcus (Johns Hopkins).

In the Philosophy section, under the chairmanship of A. G. A. Balz, these papers were given: Comment on Dewey's "Common Faith," Fritz Marti (Hollins); The Theory of Movement in Aristotle as an Application of the Theory of Oppositions, R. C. Taliaferro (Virginia); Is Ethical Judgment Essentially *a Priori?* A. F. Liddell (Florida State); Value Theory and Metaphysics, L. M. Hammond (Virginia).

The presidential address, entitled "The Postulates of Psychology," was given by J. E. Winter Friday evening after the annual banquet.

A joint program of the two sections was held Saturday morning, under the chairmanship of the president, during which the following papers were read: A Needed Emphasis in Psychological Research, C. C. Ross (Kentucky); The Worship of Universals, Marten ten Hoor (Tulane); The Psychological, J. B. Miner (Kentucky); A New Theory of Evolution, H. C. Sanborn (Vanderbilt); Unity of Science and Reduction of Nature, A. G. A. Balz (Virginia).

The afternoon program, under the chairmanship of Joseph Peterson, follows: An Item Analysis of the Maryville College Poetry Test with Respect to Sex and Certain other Personality Differences, J. D. Harris (Maryville); A Further Report on Attempts to Produce Blindness by Hypnotic Suggestion, F. A. Pattie, Jr. (Rice Institute); A Report on the Test-Retest Performance of 38 College Students and 27 White Rats on the Identical 25-Choice Elevated Maze, P. E. Fields (Maryville); A Method for Determining Student Efficiency, R. M. Drake (Wesleyan); Scoring Intelligence Tests by Weight, N. B. Cuff (Eastern Kentucky State Teachers).

At the business meeting, held Saturday after the morning session, the following officers were elected for the ensuing year: president, A. G. A. Balz, University of Virginia; members of the Council, H. C. Sanborn, reëlected, Vanderbilt University; H. M. Johnson, American University; and Marten ten Hoor, Tulane University. On the invitation from Emory University the next annual meeting will be held in Atlanta, Georgia, April 10 and 11, 1936.

The attendance at the meeting was fairly good and discussions were frequently vigorous and helpful (centered on specific points of differences). The following states were represented: Alabama, District of Columbia, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, Missouri, South Carolina, Tennessee, Texas, Virginia and West Virginia.

The Southern Society originated in the close coöperation of a few persons, some of whom gave much time to its development and perpetuity. The Council still represents this plan of paternalism by a method of frequent reëlections of its members or their representatives. There is, therefore, a concentration and perpetuation of control in the Council. Nominations of all officers still originate in the

Council. The Society seems now to have definitely reached its adolescence and should be freed of any paternalism. The writer wishes to suggest here the need of modifications in the constitution making possible a more democratic form of control of the Society. An election procedure somewhat like the preferential system now operating in the American Psychological Association would probably be satisfactory. At any rate, there seems to be no reason now why there should hereafter be any reëlections to the Council. There are already enough active young members in the Society to fill its various executive positions as these become vacant. Sensitive ears cannot be deaf to mild dissatisfactions with the present forms of election. These suggestions are made here with the kindest personal feelings toward those who have given their time in the past to more than one term in the Council, and with the belief that changes in the election procedure will effect the best advancement of the Society.

Peabody College JOSEPH PETERSON

THE TENTH ANNUAL MEETING OF THE MIDWESTERN PSYCHOLOGICAL ASSOCIATION

The tenth annual meeting of the Midwestern Psychological Association was held at the University of Kansas, Lawrence, Kansas, on May 10 and 11, 1935, with 218 registered. There were nine sections averaging 5 papers to a section. Friday evening there were five symposia as follows: "Social Reconstruction," Raymond H. Wheeler, University of Kansas, Chairman; "Clinical Psychology," L. Ackerson, Institute for Juvenile Research, Chicago, Chairman; "Vocational Psychology," F. A. Kingsbury, University of Chicago, Chairman; "Physiological and Sensory Psychology," Samuel Renshaw, Ohio State University, Chairman; "Psychology of Learning," Harvey A. Carr, University of Chicago, Chairman. A Psi Chi luncheon was held Saturday noon with an attendance of 65. Saturday afternoon Professor Wolfgang Köhler, University of Berlin, gave an address on "A Psychological Approach to Some Problems of Neurology." Professor Köhler developed the thesis that the non-process change constitutes a dynamic trace. Some time was spent comparing his theory of traces with the non-trace theory of Wheeler and Perkins.

Following the lecture, reports were heard from laboratories under the chairmanship of Professor C. A. Ruckmick, State University of Iowa.

The annual banquet was held Saturday night with Professor John J. B. Morgan, Northwestern University, Toastmaster. Following an address of welcome by Chancellor E. H. Lindley of the University of Kansas, Professor John A. McGeoch, retiring president, delivered his address on "The Vertical Dimensions of Mind."

The following papers were read: "Some theoretical implications in vibrotactile phenomena," Robert H. Gault, Northwestern University. (Fact is stressed that the ear is not the only organ involved in hearing, and conversely, that hearing supplements the sense of touch.) "Stimulus, sensation, and meaning," G. D. Higginson, University of Illinois. "The neural correlate of the affective process," Christian A. Ruckmick, University of Iowa. (The more primitive structures of the nervous system such as the plexuses are proposed as the neural basis of simple feeling, tying up the theory of feeling with the Coghill theory of progressive individuation.)

"The relation of eye-movements to varieties of clearness." Rov B. Hackman,

University of Nebraska. (More eye-movements are involved, during tachistoscopic exposures under cognitive than under attributive instructions.) "Visual factors involved in depth perception," Earl Allgaier, Iowa State College. (A common factor is indicated for binocular and monocular vision.) "Results of the training of birth injured children considered as disabled children and not as mentally deficient children," Edwina A. Cowan, Wichita Child Research Laboratory. (A report on training 14 birth-injured children.) "The influences of complexes on work curves," M. M. White, University of Kentucky. (Work decrements are greater for complex than for normal words. The Jung list of words was given to 52 *Ss* while operating the hand dynomometer.) "A comparison of questionary and performance scores of functional psychotics and psychoneurotics," Ralph Martin McGrath, Kemper Military School. (A total battery gives a closer approximation to the clinical picture than does any one test taken alone.) "Emotional factors in the etiology of stammering," Maurice H. Krout, Chicago City Junior College. (Deep seated emotional conflicts notably fixations on the oral and anal levels are found in the etiology of stammering.) "Fascist attitudes: An exploratory study," Ross Stagner, People's Junior College, Chicago. (A study of attitudes of college students and of Fascist propaganda indicates a single basic attitude underlying diverse opinions.)

"A study-attitude scale," Neal Drought, University of Wisconsin. (Various personality characteristics are found significant in relation to grade point discrepancies.) "The influence of 'subject factors' on school marks," Elmer B. Royer, Oklahoma A. and M. College. "The value of instructions in making notes," Stephen M. Corey, University of Nebraska. "Does the junior college student belong on the secondary level or on the level of higher education?" Robert A. Hardin, Nebraska Wesleyan University. "The value of a political debate," D. A. Worcester, Lee Warren, and A. J. Foy Cross, University of Nebraska. "The influence of subliminal visual and auditory stimuli on verbal responses," Lynn E. Baker, University of Wisconsin. (Judgments may be influenced by subliminal stimuli.)

"A further study of color discrimination of color-blind subjects," Alvhh R. Lauer, Iowa State College. (Color-blind *Ss* seemingly do not match colors by intensity.) "The dependence of contrast sensitivity upon the presence of other contrast borders in the vicinity of the test object," Glenn A. Fry, Washington University School of Medicine. (The separation of borders rather than the quantity of area seems to play the predominating rôle in threshold intensity for the perception of an area.) "Some factors common to flicker and gamma and real movement," S. Howard Bartley, Washington University School of Medicine. (The claims of Harrower and Newman that gamma movement is a phenomenon connected only with the process of figure formation are experimentally refuted.)

"Effect upon cochlear function of intense tonal stimulation," Glen Finch and Elmer Culler, University of Illinois. (When the tonal stimulus is interrupted about once per second it induces greater functional loss of hearing than does a continuous tone of the same amplitude.) "Studies in the influence of tensions on learning and problem solving," J. C. Stauffacher and A. G. Bills, University of Chicago. (There is an optimum degree of tension which gives maximum facilitation in certain processes of learning.) "The method of common points of mastery as a technique in learning experimentation," Floyd L. Ruch, University of Illinois. "An experiment in the law of effect in learning the maze by humans," Homer B.

Reed, Kansas State College. (On the basis of three criteria a learner has just as good or better success when punished for right choices as when punished for wrong choices.)

"Reaction-time and conditioning," Carl N. Rexroad, Stephens College. "The rôle of meaning in learning to recognize spoken sentences by their vibrotactile patterns," Louis D. Goodfellow, Northwestern University. (Experiments seem to substantiate Katz's theory that the sense of touch is the ancestor of hearing.) "Common elements in silent and oral reading," Donald W. Swanson, State University of Iowa. "Factors contributing to reading disability," Paul A. Witty, Northwestern University. "Study of good and poor reading by synchronous records of eye and voice," Grant Fairbanks and Joseph Tiffin, University of Iowa. (The eye-voice lead is held significant in poor reading.) "Relative effectiveness of summarizing and rereading," H. A. Peterson, Lois Watt, Mary Kimmell, Norma Gronemeier, and W. S. Tallyn, Illinois State Normal University.

"In defense of scientific vocational guidance," Richard Wellington Husband, University of Wisconsin. "Variations in electrical resistance of the skin in newborn infants and adults and their relation to muscular activity," M. A. Wenger and O. C. Irwin, State University of Iowa. (Increases in skin resistance seem to be associated with muscular relaxation rather than with sleep *per se* as Richter believes.) "Patterns in emotional reaction," Harold V. Gaskill and Gertrude Cox, Iowa State College. (Patterns are discriminable when co-variation and correlation are subjected to the analysis of co-variants.) "Motor accompaniments of orientational perception," Ralph E. Garner, Nebraska Wesleyan University. (Directional perception is interlocked with directional movement in at least 65% of 7200 recorded body swayings of 50 people.)

"The dynamics of spiral movement in man," Robert L. Brigden, University of Kansas. (On the basis of a field theory, helicoid, logarithmic and sigmoid spiral movements were predicted for blindfolded subjects walking in an open field. The predictions were experimentally verified.) "The fundamental components of reciprocal hand movement," David P. Boder, Lewis Institute. (By means of an oscillographic technique the variability of hand tapping movements is less in the movement as a whole than for any of the components taken separately.) Immediately after his paper Dr. Boder showed a moving picture reel of the International Congress for Psychology at Copenhagen in 1932.

"The relationship between transfer and the length of the interval separating the mastery of the two problems," M. E. Bunch and M. Rogers, Washington University. (White rats in water mazes give more positive transfer after moderate time intervals have elapsed than when short or long intervals have elapsed.) "Social motivation of sexual behavior in monkeys," A. H. Maslow, University of Wisconsin. (The dominant-subordination drive motivates more sexual behavior than do physiological drives *per se*. The sexual response is to a total situation including the sex object.) "The influence of sodium amytal on the elicitation and formation of conditioned reflexes in the cat," Paul H. Settlage, University of Wisconsin. (Conditioned reflexes were formed under a certain depth of narcosis but could not be elicited until after the effects of the drug had been dissipated.) "On direction orientation in the white rat: A comprehensive study," R. H. Waters, University of Arkansas. (Directional orientation is not achieved by fixation of a

single pathway either in the rat or in the human being, but rather, is a response to the maze as a whole.) "Color discrimination of 4 dogs, 6 cats, and 8 rats," William E. Walton, University of Nebraska. (Evidence is brought forth that the animals discriminate hues and that discrimination is attended by emotional reaction.) "Reliabilities and interrelationship of measures of blood pressure, pulse, and intelligence in college students," James P. Porter, Lillian Sparnon, and Robert Selover, Ohio University. (Low reliability correlations were found between blood pressure and pulse taken a week apart. The question is raised regarding their value for life insurance, and other predictions.) "Some needed refinements in statistical techniques," Alvhh R. Lauer, Iowa State College. (A critique of uncritical uses of statistical procedures.) "The double Hollerith card system for studying test data," Theo. F. Lentz, Washington University. "A factor analysis of Spranger's value-types," Walter A. Lurie, University of Chicago. "The effect of different methods of scoring test papers on the distribution of class marks," John A. Glaze, Kansas State Teachers College.

Professor C. A. Ruckmick, State University of Iowa, was elected president and Professor F. C. Dockeray, Ohio State University, was reëlected to the council. The next annual meeting will be held in May, 1936, at Northwestern University.

University of Kansas RAYMOND H. WHEELER

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

The New Field of Psychology: The Psychological Functions and their Government. By MADISON BENTLEY. New York, D. Appleton-Century Co., 1934. Pp. xvi, 439.

While this book is some 20% shorter than the 1924 *Field of Psychology,* and while certain paragraphs and whole pages are reprinted almost exactly, it is not a mere revision of that earlier volume. In 1924 psychology sought "to describe and to understand experience and the activities of the total organism in which experience plays an essential part." Further the organism of the psychologist was the "mind-body," the psychosomatic organism. Today psychology is concerned with certain functions of the biological organism. By fiat mind has been banished. Some twenty years ago Watson pronounced a similar edict. But neither Watson nor Bentley has developed a satisfactory systematic position. This does not mean that psychology must always be burdened with a mystical entity called "the mind." The position of the interactional organismists clearly demonstrates that a psychology free from metaphysics and physiology is possible.

The functions which psychology is to study are the five of the earlier volume: apprehending (including perceiving, remembering and imagining), executive (including action and emotion), inspection, comprehending, and finally the highest of all, elaborative thinking. The organization of the book follows this order closely, but the executive functions are given only 57 pages from the total of 351 pages of text. What is the nature of the functions? "In plain terms, your organism is *perceiving, comprehending* and *thinking-out* or *elaborating.* We shall speak of these activities as *functions.* They are functions in the very same sense as a digestive process in the stomach and the aeration of the blood are functions of the organism. It appears also that perceiving involves the eye, the brain, and other bodily structures, and so do comprehending and thinking-out involve various bodily organs and resources just as digesting and aeration do. We shall find it wholly unnecessary to assume that there is also involved another kind of organ, power or agent, which men have called a mind or consciousness" (p. 13-14). So far all is well, but the immediately following sentence is not so clear. "On the other hand, if we were to say that there is nothing going forward in our organisms as we read but the moving of the eyes, of the vocal mechanism, and of the head—as the behaviorist sometimes does,—we should absurdly refuse to observe the plain fact that *we are perceiving, comprehending and thinking*" (p. 14). What is perceiving? Earlier in the paragraph it is called a function of the organism, and is compared by analogy with the "digestive process in the stomach," which is also a function of the organism. One might ask, "If digestion is in the stomach where is perception?" The answer might be that perceiving is a function of the total organism in which no separate part has any greater importance than any other. Bentley's answer is not this, however. Judging from his chapter on the "Bodily support of the apprehending functions" perceiving is peculiarly a function of the receptors and the central nervous

system. The other functions listed are related to the central nervous system or other organs. How then can these functions be considered anything other than physiological functions, albeit more complex than digestion, secretion and the like? The author denies this most emphatically—with more emphasis than logic, it would appear. He suggests that the reader should make a list of all the things his organism has done during the morning. "As you looked it over, you would plainly see that the performances fell naturally into two great kinds, physiological functions and psychological functions." The "nature" which decides the division here implied is most probably the bias of the observer!

As further evidence that there is no real distinction made between psychology and physiology let us quote one more passage: "the ultimate reason why certain organisms can 'perceive objects and events,' . . . is that these organisms are able to repeat, in the fluid processes of their neural and other structures, the properties and energies of detached outside bits of the universe" (p. 120). If this is really what perceiving is, and presumably all other psychological functions, then ultimately such functions are nothing but out and out physiological functions.

The viewpoint and systematic position of the book might be summarized in somewhat the following fashion. Psychology is concerned with certain functions of the organism. These functions are not referred to a mind, or even to a mind-body organism, but are referred rather specifically to biological structures. In spite of this later reference it is emphasized again and again that they are not physiological functions. To the reviewer this position must inevitably lead back to a behaviorism, if not of the reflex arc variety, at least to a physiology of complex functions.

But even though one cannot accept the systematic position of the book there is something to be said for it as a textbook. The discussion of the apprehending functions is excellent. Almost equally good are the chapters on inspection, comprehending, and elaborative thinking. The chapter on acting is not satisfactory, in fact one gets some impression that it was included because after all the organism does move and therefore something must be said about it. Emotion, the second executive function, appears to be nothing other than a special sort of action precipitated by a dramatic situation and not resolvable through movement.

In spite of adverse critical comments, this book is much better for use as an elementary text than any of the eclectic primers that have poured from the presses in such volume during the past decade. Even if the instructor and student both disagree with the principles here presented, it will still serve as a stimulus to intellectual activity—and this is a characteristic notably absent from the most widely used elementary textbooks.

Indiana University C. M. LOUTTIT

The Physical Dimensions of Consciousness. By EDWIN G. BORING. Century Co., New York, 1933. Pp. xii, 251.

The aim of this book is to explicate the variables about which consciousness is organized, and to specify their physiological correlates. Introspection is here designated a method by which some of the events in the brain are revealed. The fact that every observation is essentially an interpretation is believed sufficient to sanction 'useful' speculation as a procedure to get at the nature of physiological events in the brain. Thus, a physiology may be derived from psychological fact.

The concept "dimensions of consciousness" signifies the variables in the organization of consciousness. In the present context, it symbolizes the crystallization of several movements against atomism in psychology, and thus denotes a systematic turning point from description in terms of the "hard and fast" attributes to that in terms of phenomenology.

The first of the eight chapters in the book is preliminary. The aim is (1) to point out that the psychologist must distinguish, as does the physicist, between "actual" and "real" entities; and (2) to reduce consciousness to a physiological event in order to dissipate the mystery of the problem of psychophysical correlation. In later chapters, however, the mind-body problem appears to be reified in the guise of a search for the physiological correlates of the several conscious variables.

The author expounds the four dimensions of consciousness—*quality, intensity, extensity* and *protensity* (Chapter 2). Each receives separate, detailed treatment. Three theories of intensity are presented with supporting evidence (Chapter 3). The *multiple-fiber* theory is supported by the facts of irradiation and summation in vision (Hecht); however, in a single nerve fiber, *frequency* is the correlate of intensity (Adrian). Thus, intensity seems not to be simply determined. The two theories are not mutually exclusive, and Boring 'promotes' the *volley* theory of Wever and Bray.

The organization of consciousness in the dimension of extensity entails discussion in regard to the immediacy of perceived extension, and its derivatives—size, form and position (Chapter 4). Taste and hearing are less definitely and elaborately organized in this dimension than are sight and touch, and "it is not so clear that we ought to expect extension in smells" (p. 86). Then too, the 'readiness' with which tridimensionality evolves in 'areal patterns, binocularly perceived' denotes that excitation in the cerebrum is tridimensional. The projection theory is made the basis for the explication of the psychophysiology of extensity. Although 'simple' projection is designated "pretty but improbable" and "absurdly naïve," the author commits himself to a "modified" projection theory.

Protensity, the temporal dimension of consciousness (Chapter 5) is relatively unimportant in psychology because it is "truly unidimensional" and because in referring observation to time, it has escaped being observed itself. Time, however, is directly observed in "bare protensions"—processes of brief duration—and from the standpoint of psychophysiology must be regarded "not as a mere lapse of time, but as a temporal integration, the occurrence of a definite process that takes time" (p. 138). Temporal organizations are here not too clearly differentiated, but are inferred to signify temporal relationships in the nervous system.

Quality is an extremely important category in psychology just as it always has been in chemistry (Chapter 6). Discriminable qualitative differences relate to locally distinct action in the receptor and in the brain, and thus signify a kind of 'place' theory of quality. Although the simplicity of the theory is sometimes "spoiled" in intramodal interrelationships, it is evident in modal distinctions. Only three of the modalities, however, receive specific treatment. Visual qualities seem to depend upon variation in "several (presumably three) systems of retinal excitation" (p. 163), but the large number of auditory qualities depend upon a system of many resonators with similar relationships being maintained in the cortex. The discussion of somesthesis is partial, being restricted to cutaneous sensibility. Here Boring pre-

scribes the need for more than one kind of receptor but seems unable to decide whether two or four varieties are required. The other senses are effectively dismissed with "any theory of taste would hardly be more than an analogy to the theory of somesthesia. . . . Our ignorance of the psychophysiology of smell is so great, that no discussion of this topic could be quite so dignified as silence" (p. 162).

Organization is designated the fundamental problem of psychology (Chapter 7). Its rôle in perception has already been indicated. But perceptual organizations are primary to other levels of organization. Here the author confines his discussion to three interwoven concepts—intelligence, attention, and learning—that typify the 'higher' levels of organization. Their dependence upon perception is revealed in the fact that discrimination is the 'significant' common factor. Intelligence is defined as discriminative insight, attention as reportability which thus requires differentiation, and discrimination is designated "the only possible evidence of learning" (p. 201). The nucleus for the discussion regarding the neural correlates for the higher mental processes is Lashley's law of mass action. Learning must be acquired piecemeal when the material exceeds that which can be 'squeezed into the brain' in a given moment; for, the range of consciousness establishes the limit to simple learning. The author designates his picture in regard to events in the brain reasonable but probably oversimplified.

In the final chapter of the book, the author states that one of the objects in indulging thus in highly inferential speculation about the nervous system has been to remove the mystery of an *unanschauliche Bewusstheit,* the impalpable and imponderable consciousness that will not lock horns with physical reality. Here is set forth what Titchener might have called a "point of view" but which Boring styles "the relational theory of consciousness." To state that the data of consciousness are meanings is old-fashioned. The "hackneyed connotations" of the word "meaning" are avoided by asserting that "consciousness exists as relations and exists only in the sense that relations exist" (p. 222). "It takes two events in relation to make a meaning" (p. 223). Meaning is a relation; meaning implies discriminatory reaction; thus "meaning is response" (p. 223).

The nature of consciousness itself is not clearly revealed by the introspection of introspection which yields not fact but conviction. The final appeal is to physiology. "The central nervous system is a relational organ. . . . Its typical relation is reaction, the stimulus-response relation. Response is necessarily specific and thus it is discriminatory" (p. 229). The author asserts that "if consciousness seems to introspection to be relational and yet we cannot find the terms that are related, it is because the terms are really neural. The relational theory of consciousness means merely that introspection reveals the relational nature of some of the events in the central nervous system, where selection, learning and discrimination occur" (p. 233). Thus the *unanschauliche Bewusstheit* is swept away before relational psychophysiology.

By way of critical comment, the reviewer confines himself to three points:

(1) The first concerns itself with the author's somewhat misleading use of the term 'physical dimensions' in the title. In the several chapters and in their subdivisions, the author's plan of treatment is in general (a) the psychological considerations of the topic in question and (b) that concerning the related physiological correlates. From the standpoint of content, the book is concerned with (what the

psychologist calls) 'physiology,' and is not at all concerned with "physical dimensions" in the usual sense. Only in the preface, and again briefly on page 22, does the author concern himself with physical dimensions. Here, he is for the most part prophetic with regard to the *possibility* that we may ultimately get away from the conscious dimensions and describe ideally in terms of the physical dimensions. With regard to the date of this prospective fulfillment, Boring submits that "we are not yet ready to give up the conscious dimensions" (p. vii).

(2) The second point concerns the projection theory upon which the author relies chiefly for his interpretations regarding the physiology of extensity. After pointing out the facts which make adherence to a 'simple' view of projection impossible, Boring recommends a 'modified' view of projection. The reader is disappointed in the ensuing discussion not to find more positive specification with regard to the requisite modification. What he seems to find is that the author merely reverts to interpretation essentially in terms of 'simple' projection.

(3) Finally, the specific merit of the book consists in a timely focalization of attention upon the somewhat neglected problem of meaning. Historically, this problem suffered modifications at the hands of the Associationists with the result that eventually Titchener distinguished sharply between process and meaning and allocated the latter to applied logic. But Boring will not thus side-step the problem of meaning. For him, consciousness is cognitive; it has to do with knowledge and meanings. Meaning, however, signifies two events in relation; hence, the context theory of meaning is relationally interpreted, and the *response theory of meaning* follows. Boring here embraces as the keystone in his structure the stimulus-response relation as already effectively defined by the Behaviorist. Cognitive relationships denote discriminatory reactions, and *all* conscious events are inferred by way of discriminatory response. This timely stroke of insight has a double psychological significance: it retrieves for psychology the important problem of meaning, and it unites two schools of thought—once supposed irreconcilable—introspectionism and behaviorism.

Wellesley College MICHAEL J. ZIGLER

The Moral Judgment of the Child. By JEAN PIAGET. Translated by Marjorie Gabain. New York, Harcourt, Brace & Co., 1932. Pp. ix, 418.

This is a study of the evolution of the moral judgment of children, investigated by the well-known questioning method of the author. Piaget begins by examining what is meant by 'respect' for the rules of social games (marbles in the case of boys, and hide-and-seek in that of girls), as practiced and regarded by the children. "All morality consists in a system of rules, and the essence of all morality is to be sought for in the respect which the individual acquires for these rules" (p. 1). Important questions, are, How do the children adapt themselves to the rules of the games? To what extent do they become conscious of these rules? What type of obligations result at different ages in view of the rules? The experimenter gets the children to show him how to play the game and to tell him what the rules are. He asks about all possible contingencies of the game, making no suggestions himself and appearing ignorant but serious. With small children he plays with one alone first, getting all the information he can; then with another when the first has gone, asking the same questions; then he gets the two children to play together. This control procedure

he finds unnecessary for older children. As to the second question, regarding the extent to which the children become conscious of the rules and sense their obligations to them, he asks the child whether he could make a new rule. Then "Would it be all right to play thus and so? Would the playmates obey the rule? Is the rule fair?" etc. "When you are a big boy suppose you tell your new rule to a lot of children, then perhaps they'll all play that way and everyone will forget the old rules. Then which rule will be fairest—yours that everyone knows or the old one that everyone has forgotten?" The procedure and wording will vary but the essential elements of the questions are maintained. Then the child is asked if he has always played that way, whether his father and earlier ancestors played that way. Then come questions as to the origin of rules. The order of the question is not fixed, however, but the object is to get genuine opinions.

At first, the child's activities are purely *motor* or *individual;* then in the 2nd to 5th year comes an egocentric period, each child playing his own way without regard to rules. Next comes (7-8 yr.) a stage of incipient *coöperation,* each child trying to win on some agreement as to procedure, some mutual control and unification of the rules. Ideas about rules are, however, vague. Finally the fourth period (11-12 yr.) makes its appearance, the stage of *codification of rules.* The ages designated are not to be taken strictly. In the second stage rules are regarded as "sacred and untouchable, emanating from adults and lasting forever. Every suggested alteration strikes the child as a transgression" (p. 18). In the third stage a rule comes to be looked upon as "law of mutual consent," etc. Numerous illustrations are given.

Piaget finds that the legal sense is not as well developed in the small girls as in the boys, although the stages of development noted are common to both the girls' and the boys' games. Rational rule is due to the lessening of the feeling of coercion by older children and adults (unilateral rule), and is based on mutual respect and equality. Thus the child, in growing up, comes into contact with more and more exceptions to rules—occasions for judgment—and gradually passes from the stage of control by pressure from older groups (unilateral) to that of control by mutual respect among individuals themselves. Rejecting Kant's view of respect as inexplicable from experience, we come then to what Piaget regards as the only two remaining views: that of pressure by the groups (constraint) held by Durkheim, or mutual respect of individuals held by Bovet—really successive stages in the child's development.

Telling stories of clumsiness, stealing, lying, etc., to children and evoking their judgment regarding these acts, Piaget found that younger children (6-9 yr.) tended to judge such acts in relation to the amount of damage, so that acts producing most damage should be punished most ("objective responsibility"), whereas older children (10-12 yr.) tended more definitely to consider motives ("subjective responsibility"), even though no clear line could be drawn. The younger children tended to regard lying as merely an untruth, regardless of motive, and to think it worse to lie to adults than to each other, judgments that the older children reversed. Ideas of justice, as investigated by similar methods, revealed similar differences in the two groups. Younger children would punish retributively; whereas older ones would consider conditions, and in cases not punish at all.

When the author attributes views of immanent justice (the feeling that punishment will surely follow the transgression of rules or law and that even fortuitous

misfortunes are punishments) to the young children alone one could wish that he had used ignorant adults as controls. He probably fails to note sufficiently the effect on the judgments by children of increasing intelligence with age, and of variations in punishments actually given by parents to children of different ages. One feels, also, that the data throughout are too harmonious, that the results of the experiments reflect too closely preconceived ideas of the author; that the problems need further investigation by objective methods even though the author's work is both ingenious and valuable. The author has opened an interesting and important field of experimental studies of morality in the making.

J.P.

Fools and Folly. By BARBARA SWAIN. New York, Columbia University Press, 1932. Pp. 234.

A study of the use of the terms fool and folly during the Middle Ages and the Renaissance in English and French literature, with special reference to their significance in Erasmus' *Praise of Folly* and in Brant's *Ship of Fools*. Essentially the book is of greatest interest to the student of literature, but it should hold interest also for the sociologist in its study of the fool's rôle and position in society (the court-, the clerical-, the stage-, the folk- and the private-fool); and for the theologian and moralist in the study of lists and orders of fools (reflecting the accepted sins of the day) and in the analyses of the diverse philosophies of life indicated in the various literary works reviewed.

Of interest to the psychologist is the concept of the fool's physical and personality traits, particularly the diverse and changing concepts in the latter. The fifteenth century encompassed the sinner-fool (the moralist's and theologian's) as well as the triumphant fool (Gregory's humble hero). Reflecting their different philosophies of life Brant's fool is the individual, pitiful, inadequate, and condemned sinner against order in the universe; while for Erasmus folly is universal, characterizing man's conflicting nature—but this admission of man's weakness implies his strength, so this fool is strong, gay, free, and powerful. Geographically the concepts differ also, for although "England and France had shared the idea of the fool as a symbol for erring man and the cult of the folk-fool as a symbol of fertility, French minds rather than English developed the convention of using him for a symbol of gaiety and a vehicle of gay satire, and the ideas of his gaiety and of the power of his unreason persisted longer in France than in England." The "English figure of Folly conveys a distinct threat to erring man, while the French Folie ridicules him, condemns him, yet betrays some philosophical acceptance of his existence." In Erasmus we find "the fusion of the diverse connotations of 'fool' into the figure which both condemns and vindicates man's nature." Temporally also the concepts of the fool and of folly change: by the end of the sixteenth century in England wantonness and half-wittedness had become the main characteristics, while in France the "clever varlet, rustic or clown was superseding the fool-proper as a source of amusement." The final deterioration of the concept of the fool was due probably to such influences as (1) the supplanting of the allegorical by the humanitarian points of view in literature, thus endowing the fool with traits appropriate to the country and time rather than with generalized traits; (2) the sterility and artificiality of the concept; (3) the suppression of the

Joyous Societies; and (4) the rebirth of Stoic and Epicurean concepts of human nature (thus emphasizing the good in man, rather than the evil, as the concept of the Fool had indicated largely). Today there is "no popular literary figure of the fool or folly" which compares with any of the three common interpretations of the medieval times: the sinner-fool, the critic-of-society-fool, or the merry-maker-fool.

Although there is no separate bibliography, since "the discussion . . . is concerned rather with the interpretation of accessible material than with the collection of new," the text contains numerous passages from the French and English originals. One wishes, however, that the author had included in her appended copious notes the dates not only of her secondary sources but also of her primary sources.

The study is comprehensive within the field it sets out to cover (the literature of Erasmus' time) but we must remember that it omits the use of these concepts in such other fields as the drama and, more primarily, the meaning of these concepts by the man-on-the-street. Since the literary expressions must have reflected these latter meanings we may consider this study a valuable approach to the whole topic, and of special interest to the social psychologist.

University of Maine LILLIAN M. HATFIELD BRUSH

The Eugenic Predicament. By S. J. HOLMES. New York, Harcourt, Brace & Co., 1933. Pp. xi, 232.

The author is keenly aware of the controversies between the environmentalists and the hereditarians as to the methods of improving the race. He takes the general position that whether much or little can be done to this end "enlightenment must come first" (xi). His own judgment is that the trend of evidence resulting from "the progress of knowledge in this field has materially strengthened the position of the eugenist." In this book he devotes especial attention to "certain key positions about which the battle between eugenists and their opponents has been concentrated." He frankly holds the results to indicate "that if brains are not inherited there is small chance of acquiring them" (p. vii).

In the chapter on "Some Biological Preliminaries," Holmes says that "most of the developments which form so striking a feature of the recent history of mankind have to do with the culture and institutions of man instead of his inherited qualities" (p. 4). This has not, however, improved the biological qualities of the race; indeed, these qualities, he holds, have become burdened with numerous defects. "Nature does not care a fig whether her children advance or slump backwards. All that she is interested in is that they go on somehow and transmit their precious germ plasm" (pp. 4, 5). Nature is by no known condition bound to go upward; only wide divergences in hereditary qualities afford the bases for selective breeding. On this principle breeders have made marvelous developments in plants and animals. Selection is of little value without a great deal of hereditary diversity in genes. "On account of his diversified heredity, man is . . . a highly modifiable animal" (p. 9). Proper eugenic precautions are, however, necessary for the improvement of the native qualities of man.

Some of the numerous defects in man's heredity, of which it is desirable to get rid, are indicated. "As to the sins of the fathers we may confidently assert that the offspring escape scot-free from almost all of them" (p. 11). While Holmes recog-

nizes the benefits of favorable environment, he is here interested in the "numerous inherited afflictions which have no conceivable connection with parental misbehavior," concerning the elimination of which much information of great value has accumulated. He estimates, from already known hereditary defects of the eye, that all the hereditary defects of this single organ alone probably amount to 1,600. Add to this the similar defects of the ear, the heart, and the numerous other organs as well as more general hereditary defects, and one cannot doubt that there is great need for eugenic work and for general knowledge of sound genetic principles.

Data are presented both in the text and in a 34-page appendix to support the author's contention that both in feeble-mindedness and in neural weaknesses and insanity of various types the effects of hereditary factors are very great. Much of the evidence is of a sort supplied from recent researches on comparisons of defects in numerous pairs of identical twins, on the one hand, and those of fraternal twins, on the other. Similar methods are used also to show the importance of hereditary factors in the production of superior ability. Criticising the positions taken by various present-day environmentalists, Holmes says: "The work of the mental testers has been by no means lacking in hostile critics [as is the case also with eugenics]. There is a looseness about the whole business [of testing] that is particularly irritating to a person of scientific training, but those who unreservedly condemn this work impress me as lacking in insight and balanced judgment" (p 77). Holmes' methods, however—and he is, of course, not opposed to the use of environmental factors in individual development—are so basic as to afford good grounds for a critical evaluation of the controversies regarding the results of mental-test comparisons of identical and fraternal twins. He indicates clearly various adverse tendencies in the human race as to the sources of the birth supply, the most able persons failing even to supply offspring enough to maintain their present numbers. The general outlook, therefore, even though there is encouragement in the lesser death-rate in the children of the better endowed parents, and in other such minor factors, is not one to foster optimism. Yet, due to possibilities of enlightenment as the dangers grow greater, the situation is not regarded as hopeless.

The criticisms of certain writings, even by geneticists like Raymond Pearl and H. S. Jennings, are illuminating to readers who have not kept abreast of developments in genetics and eugenics.

In the closing chapter on "What Can We Do About it?" there is much information of value in the form of statistical data on the practices of other countries. Throughout the book the author adheres closely to established biological principles, mainly based on Mendel's law of heredity and the known facts of biological selection. The book is valuable to psychologists and educators, and (may we venture to suggest) to sociologists. Fortunately it is not too technical for the intelligent layman.

J.P.

Das Gefühl. By ARTHUR WRESCHNER. Leipzig, Quelle & Meyer, 1931. Pp. xii, 193.

This volume presents a systematic survey of the psychology of feeling. There is a somewhat cursory historical introduction, which traces the development of affective theory from the scholastic philosophy of the Middle Ages to the psychologies of

the present. The theoretical problems of the essential nature of feeling and its relation to other mental processes occupy the author's attention almost exclusively. There is no reference to the experimental literature.

The book, according to the author, is devoted wholly to the scientific investigation of the affective life. His conception of science is apparently rather broad, for he includes a great deal of highly theoretical, non-experimental material, while of science in the narrower sense of laboratory research there is a disappointing lack. Except for the citation of a few of the studies on the bodily expression of emotions, the experimental literature is almost entirely ignored. There is a good discussion of the classical theories of feeling which have been on the carpet for the last half century (Lipps, Wundt, Stumpf, Lange and James), but of the more recent developments in the psychology of feeling (the highly controversial literature on bodily concomitants, the extensive studies of affective conditioning and the important work of the new Leipzig school) there is practically nothing. The contributions of Cannon and Pavlov are omitted.

In Part I, the author adheres rather closely to the conventional treatment of his subject. His criteria of feeling are the time-honored ones of subjectivity, dependence upon sensory or ideational contents and central conditioning. The methods of studying feeling are briefly described and a few of the results noted. In a chapter on the conditions of feeling, Wreschner explains the effects of (1) attention, (2) expectation, (3) adaptation, (4) contrast, (5) fatigue, (6) the will, (7) bodily state, (8) drugs, stimulants and poisons, (9) the total conscious state, and (10) the individuality. He clings to the theory that attention to a feeling weakens or destroys it. The influence of the 'will' is reduced to a matter of attention.

Part II contains more original material. After devoting 5 chapters to the classification of feelings and description of specific types, he states his own affective theory. Feeling and intellect, he believes, are not opposed to each other, as psychologists have so often maintained, but are very closely interdependent. The intellect supplies feeling with its motive—sensory or ideational. It also makes possible the qualitative richness and differentiation of feelings. The intellect, on the other hand, is beneficially influenced by feeling in many ways. Feeling aids both in impressing events upon us and in reproducing them in memory. Feeling determines the association of ideas and stimulates the intellect to creative activity. Furthermore, all mental processes and contents originate in feeling. Not only the advancement of knowledge, but the development of the inner mental life is controlled by feeling. Thus feeling becomes the primary root and determinator of the entire psyche.

The immediate correlate of this radically emotionalistic epistemology is a strictly emotional theory of the will. Feeling provides both the motive for the inception of the volitional process and the end-phase—the joy of success or the unpleasantness of frustration. In the last analysis, there is no essential difference between will and feeling. Pleasantness involves a 'striving-toward' and unpleasantness a 'striving-against.' "I feel pleasantness," means *"ich verlange";* "I feel unpleasantness," means *"ich verabscheue."* In every feeling there is imbedded a *"Tendenz."* Hence feeling contains within itself the essential element of will and the two thereby become practically identical.

Wreschner classifies the theories of feeling under three headings: (1) *physiological theories,* which regard feeling as a sensation, (2) *logical theories,* which

derive feeling from the intellect, and (3) *psychological theories,* which treat feeling as an ultimate. He rejects the first and second types of theories. The physiological theories fail to recognize the essential nature of feeling and the logical theories confuse the motive for feeling with the feeling itself. Some 16 pages are devoted to a criticism of the James-Lange theory. The psychological theories are divided into 3 sub-groups, distinguished by the number of qualities assigned to feeling. Theories which admit an indefinite number of qualities are represented by Lipps and Krueger; theories that postulate several pairs of qualities, by Volkelt and Wundt. Both of these sub-types are found inadequate. In concluding the chapter, Wreschner aligns himself with Lehmann, Külpe, Titchener and the "majority of psychologists" in supporting the third sub-type of psychological theory—the theory that feeling is a separate mental process, coördinate with sensation, consisting of two contrasting qualities, pleasantness and unpleasantness.

The book as a whole is quite readable; the style, lucid and direct. Lengthy theoretical controversies have been avoided. Although it cannot be recommended as a complete and up-to-date treatise on affective psychology, it should prove helpful to the student who desires a fairly concise critical survey of affective theory.

Rockland County Collegiate Center E. FRANCES WELLS

Prediction of Vocational Success. By E. L. THORNDIKE, ELSIE O. BREGMAN, IRVING LORGE, ZAIDA F. METCALFE, ELEANOR E. ROBINSON, ELLA WOODYARD, New York, Commonwealth Fund, 1934. Pp. 284.

The Commonwealth Fund financed this study, begun in 1921, of the contribution of psychological tests and items from the school record to the prognosis of vocational success. The results here presented are certainly not favorable to extensive use of tests; and the vocational psychologist reading this book must feel ill-rewarded for the labor of examining its descriptive detail and extensive tables, unless the authors are justified in their methodological assumptions.

The tests given were of three kinds, including arithmetic and reading comprehension (combined later into a single score of so-called general intelligence), clerical tests, and tests of mechanical adroitness (Stenquist for boys, I.E.R. Assembly for girls). Scores were obtained from over two thousand school children averaging about 14 yr. of age, these children being a sample of the eighth grade children in New York City schools. Their success in later educational endeavor and in occupational adjustment was studied in relation to test scores and items such as age at leaving school, school attendance, and scholarship and conduct marks.

Thorndike's conclusion is that vocational guidance by means of objective measures of what an individual does at age 14 yr. is likely to prove relatively futile, because prediction of earnings, of level of work reached, and of satisfaction in that work as of ages 18-20 and 20-22 yr. is very unsatisfactory. This result he attributes to three possible causes: either the tests and other measures of traits used in the study are not adequate (which he rejects, at least in part), or the individual changes considerably in his characteristics after the age of 14 yr. (which he considers unlikely), or the employer does not reward the individual relative to the grade of service rendered (which he seems to favor as a cause).

The assumption which Thorndike adopts is that if a person stands high in a given trait, *e.g.* "abstract intelligence," he should stand high in occupational status, as

measured by earnings, or level of work reached. In our opinion this is not to be expected, even if we agreed that success should be measured in such terms as earnings or level of work (by which standards society generally does measure success). The proof that the assumption is wrong has witness in the studies of occupational ability patterns recently reported by Trabue. We quote from *Occupations*, Vol. XII, No. 8: ". . . success in a given job is dependent on the possession not only of the right combination of abilities, but also of the correct attitudes, likes and dislikes, interests, personality traits, social behavior and the like. Among all these, perhaps the individual's abilities and skills are most readily subject to objective measurement, and yet very little is yet known about the exact amounts of different kinds of ability actually possessed by those persons who are working successfully and happily in different kinds of jobs. . . . For each occupation there seems to be required an optimum degree of each quality and this optimum is rarely the maximum degree" (p. 40).

This consideration alone assures the failure of a program which depends solely on correlation technique for its most significant results. Also, it is doubtful if any program of tests which includes academic ability and accomplishment as largely as this study did can give positive conclusions about vocational ability. The assertion of Thorndike, that the data obtained are representative of what the counsellor would obtain today, seems unwarranted. The more reliable tests (the assembly tests are not reliable enough for prediction) are certainly of significance for educational prognosis, but have little in common with the occupations for which they were supposed to predict success. It is admitted by the authors that if they had had the measures of interest, coöperativeness, etc., now available their predictions might have been improved.

The vocational counsellor will perhaps object that the study did not include the personal impression which the individual makes on the adviser, which can be expected to supply information of considerable value. The answer given is that if this is so, it has still to be demonstrated. Where counselling accompanied the use of tests, as it did in Earle's study, the correlations between test scores and employers' ratings of satisfactoriness were a little higher than here. The conclusion is thus forced on the reader either that the employer estimates the value of the individual in other terms than the counsellor, or that the ideal counsellor does not exist.

For clarity of presentation of results, and for completeness with respect to follow-up of a large percentage of the original subjects, this study may serve as a model. It is regrettable that the authors did not broaden the conception of counselling to include more than a few tests of aptitude and school items.

University of Texas W. H. BRENTLINGER

The Psychology of Adolescence. By KARL C. GARRISON. New York, Prentice-Hall, 1934. Pp. xxi, 377.

According to the Preface, this textbook on the psychology of adolescence aims to introduce the student to the basic experimental studies, as well as to supply valuable information to "the many students in college classes who are still in the latter stage of adolescence" and to "those who are intrusted with the care and guidance of adolescents." The first chapter defines "The Adolescent Age." Chapter 2 treats of physical development, Chapter 3 treats of mental development, and the rest of the book (fourteen chapters) is devoted to the topics of social development, personality,

mental disturbances of adolescents, juvenile delinquency, guidance, and the hygiene of adolescence. The presentation is elementary.

The best that can be said of this book is that it is, on the whole, sound. On several points, however, the reviewer must take issue. It is probably exaggeration to assert that "The problems of adolescent mental hygiene have their inception in most cases in the early fondling and feeding activities of the growing newborn baby" (p. 263). We are willing to warrant that this statement cannot be statistically verified for any normal sample. The author's optimistic view that, through sports and games, the adolescent child "learns to place the welfare of the group above individual welfare" (p. 109), requires the acceptance of a theory of transfer of training that is contradicted by practically all the available scientific evidence. Scientific evidence is similarly neglected in the author's estimate of the value, for the development of desirable personality qualities, of "a well-grounded and firmly rooted religious attitude" (p. 259). The definition of intelligence as the ability to adjust oneself to the environment (p. 41) seems a better criterion of feeble-mindedness or *lack* of intelligence, than of the type of intelligence measured by generally accepted tests. The whole chapter on mental development is, in fact rather disappointing; in a field in which experimental work is especially abundant, the only two graphs presented are purely theoretical (pp. 45 and 51). Particularly disappointing is the treatment of the topic of heredity and environment. The author stresses the indistinguishability of the contribution of nature and nurture—"one can never determine . . . the relative values of these two forces" (p. 64). But—we wish to suggest—the nature-nurture problem is not concerned with "relative values;" it is concerned with *relative effects*. The whole science of genetics (not to mention the well-known psychological work of Freeman and Burks) is proof that the nature-nurture problem is a real problem, susceptible to experimental attack. As well say that the contribution of a pitcher in a baseball game cannot be evaluated, since there are nine men playing coöperatively on the team!

It is no special criticism of this book to say that its treatment of adolescent sexual adjustments is, as in most books, conventional, timid, and ineffectual. Authors, in general, prescribe "information" and "physical activity;" while adolescents are thinking of "dates," petting, contraceptives, "feminine hygiene," sexual experience, companionate marriage, and the problem of normal sexual intercourse when love seems present but marriage is economically unwise. Authors obviously aim at the *restraint or prohibition* of sex. The adolescent is looking for the hygienic and safeguarded *exercise* of sex; and some adolescents are doubtless discovering their dangerous way to such exercise—but certainly with little or no aid from the usual chapter on sex in texts on psychology.

The present book sadly lacks contacts with current events. Thus, the whole chapter on "Moral and Religious Growth" fails to make any mention of the significant religious changes in Russia, Mexico, and Spain. A review of the "youth movement" in Germany is confined to a one-paragraph quotation (p. 120). The breakdown of home influences with increasing urban growth and the waning position of the family, and the changes in national psychology under the stress of persistent economic depression, are completely untouched. Such current fields are, of course, difficult to discuss authoritatively; but failure to offer any discussion at all is to dodge present reality, and to gain scientific validity at the expense of interest and current relevance.

A few technical errors require mention. The percentages in Table I (p. 5) add

up to 187 and 247.88, respectively! On p. 30, on the first line, omit "variation in." On p. 288, the reference should be to C.R. (not R.C.) Shaw. On pp. 291 and 292, the reference should be to Lowrey, not "Lowery." In several places the author's phraseology is careless or inelegant.

It is the thankless task of a reviewer to point out the shortcomings of a new work. As has been indicated, the book under discussion is not without its faults. The book nevertheless is, on the whole, a sound, conventional discussion of the field which it aims to cover.

University of California HERBERT S. CONRAD

Manual Skill: Its Organization and Development. By C. W. Cox. Cambridge, Cambridge University Press, 1934. Pp. xx, 247, 29 tables and 53 figs.

This work comes from the National Institute of Industrial Psychology, London, England.

Time scores for assembling and stripping a small electric lamp socket during 10 days of practice were correlated with measures and estimates of intellect, mechanical aptitude, ability in school, ability in games and drill. The author states that he has succeeded in isolating unitary, independent, functional elements as follows: (1) "A general factor which functions throughout the whole of the activities with the possible exception of the simplest manual operations;" (2) "A mechanical factor which functions only in those activities which involve a mechanical problem;" (3) "A routine manual factor . . . restricted to simple manual tests;" (4) "Two small additional factors each restricted to a pair of simple manual tests;" and (5) "A factor peculiar to each operation." The saturation of each test with each factor is determined by the use of Spearman's tetrad difference and the partial correlation technique.

The sampling is admittedly small (59 girls, 60 boys and 70 adults). The experimental groups contained 39 *Ss* or less. The correlations and charts are based on only 60 cases at best, many on 30 or less. Moreover, as there is no attempt to show the relative normality and dispersion of the groups comparisons are very hazardous.

The analysis of factors undertaken statistically in Parts II and III is not well integrated with the analysis of processes undertaken subjectively in Part IV. The seven processes listed under the "mechanical" factor are so different that one would not be justified in claiming this as a unitary psychological factor. This same dilemma is found in the case of the "routine assembly" factor. In Part IV, apprehension, reproduction, and education are listed under both "mechanical" and "routine" assembly; hence these two are not wholly independent.

In this controversial field, nearly all workers admit the statistical isolation of rather complex factors by Spearman's technique. The presence of these factors depends upon the types of material used and the kinds of errors involved. The emergence of 'ultimate elements,' such as those claimed, will depend upon much more careful controls—as was the case in the isolation of chemical elements to which the author refers. The use of such words as 'ultimate,' 'unitary,' and 'elementary' in this type of analysis is scarcely justified mathematically according to Burton H. Camp, Piaggio, and others. The analysis, moreover, lacks the careful evaluation of previous knowledge and interest. Although these two factors are mentioned as being very important, they are not considered in the factor analysis.

The nature of the curves, of zeros, and of the factors found might change considerably if amount-per-time scores were used instead of time for one repetition. This would also reverse the conclusion that there was a tendency toward smaller individual differences after practice than before. The relation of the variability of a subject with his amount of practice is not given. One might expect that, with practice, subjects would become less variable on such routine tasks. If this is the case, the conclusion that reliability does not increase with practice should be questioned. The conclusion that reliability is not related to complexity of task is not clearly proved, since the amounts of practice before the experiment are not known, and the intervals, during the experiment, between repetitions of simple items were not the same as those between complex items. Moreover, the term complexity is nowhere clearly defined.

The work lacks an analysis of changes with practice and training; in general, the earlier practice effects are probably cognitive and the later manual. A factor analysis of the final trials might throw considerable light on this problem. If one wishes to make comparisons accurately, the processes involved must somehow be reported in equivalent units. This study does not attempt to indicate how problematic the "mechanical" assembly was to each subject and what processes were actually involved.

The book has the merit of examining in a systematic way the results of certain observations and measures. The findings indicate syndromes which point out the possibility of more analytic measurements of problematic and manipulative activities. The differences between more repetition and guided training seem very significant.

The book does not attempt to present immediate practical contributions such as standard test procedures, or adequate norms for vocational guidance, but a thoughtful worker in the vocational field may find a clarification of concepts in Part IV.

University of Michigan EDWARD B. GREENE

The Ape and the Child: A Study of Environmental Influence upon Early Behavior. By W. N. KELLOGG and L. A. KELLOGG. New York, McGraw-Hill Book Co., 1933. Pp. xiv, 341.

This book is a contribution, from an unusual angle, to the ever-interesting heredity-environment problem. There have been studies of twins of identical heredity reared in different environments and of twins possessing different heredities reared in similar environments, and we have some knowledge of the effect of a radically different environment on human offspring from the relatively few cases of children reared with wild animals, *e.g.* the "wolf children" of India. Here we have a study of the effect of the same environment on two organisms of different species, and hence of widely different heredities.

The authors had acquired from the Anthropoid Experiment Station of Yale University at Orange Park, Florida, a female chimpanzee aged 7½ mo. Their own son Donald was 10 mo. of age. The two, ape and child, were given the same training and environment, as far as possible, for 9 mo., that is, until Gua, the ape, was 16½ mo. old and Donald 19 mo., at which time the experiment was discontinued and Gua was returned to the Experiment Station. "According to our plan," the authors write, "the animal subject was to be fed upon a bottle, clothed, bathed, fondled, and given careful human treatment in every phase of its daily existence.

It would be placed in a peramulator and wheeled. It would be induced at the proper time to walk upright as the human child is assisted in this process. It would learn to eat with a spoon as soon as it was able to eat at all by itself." It may also be added, an attempt would be made to teach the ape the pronunciation of words, as the child was taught.

The first chapters of the book are devoted to the simpler activities of the two subjects, *i.e.* to eating, sleeping, muscular coördination and walking, the senses, and play. The last half of the book is concerned with a comparison of the subjects in regard to social and emotional behavior, learning, memory and recognition, intelligence, and language. In comparing the intelligence of the two subjects, besides, incidental observations of behavior, the subjects were systematically given the Gesell tests for Pre-School Children. The results in terms of the numbers of tests in which each subject was superior to the other gave Donald a score of 23, and Gua a score of 15. It would then seem that Donald was the more intelligent, but when curves based on the total number of Gesell tests passed were made, it was found that the two subjects were remarkably similar, Donald having only a slight advantage. In the fascinating "suspended cookie" test and the "hoe experiment," in which the subjects were to rake in with a hoe a piece of apple, it was Gua who proved herself superior. In the "suspended cookie" test, the very interesting fact appeared that whereas Gua attained the cookie whatever may have been the position of the chair in relation to the place of the cookie, Donald had learned certain stereotyped ways of moving the chair and consequently did not get the cookie when the position of the chair with reference to cookie was changed. His behavior strongly suggested that of Thorndike's cat when the place of the loop in the box was altered. In regard to the articulation of human sounds the authors state that the superiority of the child in *vocal imitation* stands as a striking difference between the two subjects. The meager results with Gua should not, however, argue against the ape's speech ability, since the time of training was so brief. In the comprehension of words there was no significant difference between the two subjects.

In the concluding chapter the authors summarize the results under the headings, Differences Favorable to the Child, Likenesses, and Differences Favorable to the Ape. It is of interest that differences favorable to the ape which can be reasonably attributed to the ape's unusual (*i.e.* its human) environment are considered to be of slight significance. They include such items as greater coöperation and obedience, tendency to kiss for forgiveness, her skill in opening doors, and her striking ability to eat with a spoon and drink from a glass.

It is clear from even a casual reading of the book that the experiment was no easy task; the constant care and supervision, the taking of measurements, and the giving of tests of many different sorts, obviously constituted a full-time job. The authors are to be commended for having done it so well and with full consciousness of its limitations and unavoidable weaknesses.

University of California CHARLES H. HONZIK

Die Grundlagen einer experimentellen Bildungspsychologie. By WERNER STRAUB. Langensalza, Herman Beyer & Sons, 1931. Pp. 104.

The aim of this very much condensed work, which is itself somewhat of a review of psychological history, is to define the problems and determine the possibilities

of educational psychology, which, in the opinion of the author coincides essentially with psychology *überhaupt,* instead of being merely applied psychology. Beginning with a critical discussion of Meumann's experimental pedagogy he shows how the development of educational psychology was strictly conditioned by the advances in general psychology, while on the other hand this general advance was partly conditioned by the inadequacy of psychological methods for the solution of the educational problem, and for the foundations of educational theory.

The problem of education being the modification of the individual by means of certain influences, it becomes necessary to know not what the individual is in general but rather, in concrete cases of education, what he is in particular. Straub recognizes that the attempt to solve this aspect of the educational problem involves typology and characterology as developed by various methods, which lead eventually to a study of the origin of types in the studies of child psychology and genetic studies in the broadest sense of the word. A form of education specifically adapted to types will not meet the requirements of the educational problem unless the fundamental psychic forms are invariable in their structure and without plasticity. In any case the problem of educability of single functions must be supplemented by an investigation into the educability of functional structures and ultimately of the total functional structure. As a matter of fact typological investigation has revealed an enormous amount of plasticity. This plasticity Jaensch has endeavored to explain in the case of fundamental forms by his theory of "competition of plans of structure," these forms becoming modified under the influence of environment. The immediate working out of the concept of totality in genetic psychology eventuates in the attempt to discover general phases of youthful development.

The inadequacy for the purposes of pedagogy of a psychology on the model of the inorganic natural sciences together with the attempt on the part of psychology to secure a broader foundation, have led to a separation of child psychology from pedagogy. Before this emancipation took place the science of childhood was liable to be replaced by the science of the school child. Nevertheless, this emancipated child psychology has come in its turn to the consideration of pedagogical questions, to teaching and education as realities which are effective in psychic development; so that one can not regard ontogenesis as a mere unrolling of capacities but rather as a product of inner and outer factors, the separation of which is only abstractly possible.

Vanderbilt University HERBERT SANBORN

Bergson. By V. JANKÉLÉVITCH. Paris, Librairie Félix Alcan, 1931. Pp. viii, 300.

This book is the 8th volume in the series, *Les Grands Philosophes,* edited by F. Palhoriès. It is based upon an article published in the *Revue de Métaphysique et de Morale,* 1928, under the title, *Prolégomènes au bergsonisme,* and is essentially an elaboration of this earlier paper. The original article has been wholly incorporated in the book, constituting approximately one half of the volume. To this, much new material has been added. The entire chapter on "Mind and Body" is new, as is most of the chapter on "Life." Paragraphs containing supplementary illustrations and discussions have been frequently introduced.

Jankélévitch's *Bergson* presents a sympathetic interpretation of the more important Bergsonian concepts rather than a critical and comprehensive exposition of

Bergson's philosophy. There are 5 chapters, each devoted to the discussion of a major concept. Chapter I, entitled "Organic Totalities," deals with the Bergsonian dualism of organism and mechanism, spiritual *versus* material, and describes the distinctive characteristics of organic totals which lead Bergson to reject all theories that attempt to explain life in terms of mechanistic concepts. An organism is always a unity, a total that is not equal to the mathematical sum of its parts. It is impossible to reconstruct organic totals by mechanical methods. The attempt to do so involves *l'illusion rétrospective*—the confusion of characteristics which are recognized *a posteriori* as logically simple with actually primitive, chronologically simple, characteristics. In the succeeding chapters, Bergson's theories of freedom, mind and body, life and concepts are expounded. The persistent dualism of Bergson's thought is emphasized.

This volume deserves a significant place in Bergsonian literature as a careful and sympathetic interpretation of the master's philosophy. Perhaps the greatest compliment that may be paid to it is the tribute of Bergson himself, as expressed in an introductory letter:—"Your exposition is not only exact and precise," but, "above all, it also shows a remarkable understanding of the doctrine and an intellectual sympathy."

Rockland County Collegiate Center E. FRANCES WELLS

Environment and Growth. By BARKEY S. SANDERS. Baltimore, Warwick & York, Inc., 1934. Pp. xviii, 375.

Three hundred pages packed with authenticating references to a closely printed bibliography covering sixty-five pages, an extensively detailed table of contents, and a full index would be enough to attract and hold attention no matter what the subject. But when that subject concerns the much discussed problems of environmental influence upon physical and mental development and the relationship of it to heredity, the psychologist, who cannot possibly take time to read all the biological literature involved, finds his attention held and an attitude of eagerness aroused.

The range of subjects treated is as inclusive as the literature: differential growth of the fetus and the effects upon it of countries, seasons, economic cycles, wars, famines, and the like; differential growth in infancy and the effects of a great variety of environmental factors; differential growth through childhood and adolescence and the influence upon it of a host of factors. The thesis is in substance that the trend of systematic and experimental studies is definitely against the hereditarian views of the Pearson school, that there is much differential growth attributable to environmental influences, that there are indications of a phylogenetic change in size as a result of persisting environmental conditions, that the evidence suggests some transmission of acquired characters, and that if all this is probably true of organic growth it must be as much or more true of psycho-social behavior.

Criticism of such a work would be difficult even for a highly trained biologist unless he knew all of the literature used and quoted. Its strength or weakness depends upon the validity and reliability of the studies referred to. If these are inaccurately reported or not properly interpreted then the argument may be faulty. But for the psychologist who cannot hope to be freshly read in all this literature the book presents a mine of information and a welcome collection of reference material.

As a guide to the literature, it seems to be incomparable. It is not the kind of book one reads from cover to cover with unflagging interest. The style is sometimes difficult. The high condensation and the wealth of references put the book, for the reviewer at least, more in the class of a handbook or a closely written encyclopaedia. As an index it should long be of service. The very thought of the amount of labor necessary for such a compilation is appalling.

Indiana University EDMUND S. CONKLIN

Modern Psychologies and Education. By CLARENCE E. RAGSDALE. Introduction by M. V. O'Shea. New York, Macmillan Co., 1932. Pp. xi, 407.

This is an elementary book written to meet the needs of students in teacher-training institutions and of teachers who are able to get only a minimum acquaintance with psychology. The author also devotes considerable space to the evaluation of various education problems. The book stresses the need of approaching problems from the point of view of scientific attitude rather than from that of theorizing, and it brings out the differences between these two approaches.

Along with the objective point of view stressed in the book is an emphasis on overt social behavior. The author contends that psychology should begin in elementary schools and points out that under the present conditions relatively few persons become acquainted with psychology. He rightly holds that the popular view of behavior is so different from that of scientific psychology that even college students experience considerable confusion when first they come in contact with scientific methods of the explanation and study of behavior. It would be valuable, undoubtedly, for psychologists, dealing with the objective aspects of behavior, to introduce work in psychology in connection with biology and hygiene at a much earlier age than psychology under present conditions begins to impress the youth. There is little doubt, as the author points out, that such an early gradual acquaintance with the objective facts of psychology would yield valuable results. At present even most public school teachers have very little effective knowledge of psychology. Suitable data, charts, and directions for observations and for simple experiments would have to be properly organized and simplified.

A survey of the contributions of modern psychology is made with such interpretations as the author sees fit to make. It is unfortunate that in this part of the book, even though the author seeks to point out the established facts in psychology, he bases his interpretations mainly, if not wholly, on statements of theories and hypotheses without sufficiently acquainting the reader with the experiments devised to test such hypotheses and with the results obtained.

Instead of contending, as he does, that learning requires "double stimulation," the author could well have said "multiple stimulation." He seems to be unacquainted with many of the newer developments in the field of learning both on humans and on animals, and several of his criticisms are of older views. Certainly not all behaviorists make the "conditioned reflex typical of all learning." Likewise his characterizations of certain 'schools' of psychologists are not accurate, but differences are exaggerated, or likenesses are overlooked. Thorndike's important recent work on frequency and recency factors in learning is not mentioned. The justification for this is probably that Thorndike's earlier emphasis on these factors is also not considered.

While there is nothing new in the book for persons acquainted with psychology it must be said that the point of view of the author is in general rather sound with such exceptions as have been noted. The book is, of course, not intended as a source of information in psychology.

J.P.

The Fundamentals of Psychology. By W. B. PILLSBURY. 3rd ed. New York, Macmillan Co., 1934. Pp. xii, 663.

This new edition incorporates most of the material and in many places duplicates word for word and illustration for illustration the original 1916 volume. The author again attempts to cover the entire field of psychology in one book and the work makes a good introduction for a more thorough study of the science.

Some new material has been added and under most topics a paragraph has been inserted setting out the *Gestalt* point of view. In the discussion of "Unit of Experience" (a new title) the *Gestalt* position that appreciation of an object comes immediately and cannot be analyzed into parts is accepted, while for perception the theory is rejected because it does not give sufficient weight to the importance of old habits and experiences.

The author recommends that the use of such terms as 'will' and "instinct' which have gathered about themselves much confusion and controversy be dropped from scientific discussion. 'Will' is treated under 'volition' and, although the treatment is handicapped by terminology derived from a dualistic ancestry, there is a logical presentation of purpose and ideals as the summation of past experiences, which seems to create a set in the individual and direct behavior along lines in harmony with the present environmental and physiological conditions.

The James-Lange theory of emotions is rejected largely upon evidence derived from Cannon's experiments. The rôle of muscular activity as coloring emotions is, however, admitted. The author arrives at a middle position, employing but two types of emotion, one violent but of short duration, the other of greater duration but of milder intensity. In the latter type mental antecedents determine the mode of emotional expression induced by any stimulus.

The chapter on reasoning remains largely speculative. Arriving at meanings, as the important factor in reasoning, has given way to a discussion of problem-solving as a trial and error procedure or the chance formulation of a concept. The discussion runs over into a treatment of "Imagination and Dreams" which are given a common origin with reasoning but differ from reasoning in the extent to which they are recognized as reality. The Freudian theory of the unconscious is discredited. Recall is treated from the point of view of association and Semon's term 'engram' is employed to designate what is retained within the organism and subject to recall. 'Engram' adds nothing by way of explanation.

Behaviorism is rejected as a system of psychology because it treats man as a mere mechanical reacting mass, whereas man is active and self directing.

Pillsbury has presented in a clear, readable style the more important theories of the science. The most serious criticism to be made of the book is the lack of specific references. Many arguments and conclusions are drawn from specific experimental studies but the reader is not supplied with the source reference and sometimes not

even with the name of the investigator. This absence of specific references greatly minimizes the professional value of an otherwise enticing volume.

George Peabody College H. W. FENKER

Differenzierungserscheinungen in einigen afrikanischen Gruppen: Ein Beitrag zur Frage der primitiven Individualität. By S. HOFSTRA. Amsterdam, Scheltema & Holkema, 1933. Pp. 214.

This work was suggested to the author by S. R. Steinmetz and is essentially a critical inquiry into methods of research on primitive people. As is well known, Steinmetz advocates what might be called the psychological method of individual differences in ethnographic investigations of groups (see, for instance, his *Anleitung zu einer systematischen Ermittelung des Individuums bei den Naturvölkern*). The author critically analyzes the various points of view in contemporary research on primitive man. One central problem inside sociological ethnographic research seems to be this: How far is it possible to get at the study or the "laying-bare" of the individual, as such, in the midst of the dominance of customs, traditions, taboos, etc., of primitive groups upon the individual?

By examples from the literature, the author illustrates the various angles from which this problem has been and is being attacked. We find the extreme collectivistic social point of view in the *Durkheim-Levy-Bruhl* school and the rather extreme individualistic point of view of Americans like Boas and Lowie, as well as in the school of Steinmetz and his co-workers, with William McDougall somewhere between the extremes.

The author offers a very thorough and interesting discussion of concepts of sociology, such as individuality, the group, collectivism, *Persönlichkeitsbewusstsein*, etc., and he states his problem as follows: "Where do we find individual differentiations among primitive people and of what nature?" In looking over the field, it is found that very few ethnographic descriptions have good psychological material, while the institutional and static side of primitive life is generally well portrayed. It is apparent that the interest of the individual ethnographer has been rather decisive in determining what material he will center his attention upon, and that a general method in this field, so to say, is entirely lacking. In the selection of his own material for scrutiny, Hofstra, therefore, limited himself to material from a rather homogeneous field, choosing the following African tribes: *Thonga, Ba-ila, BeVenda, Bafioti, Dschagga, Ashanti, Ewe, Kpelle, Lango.* He arrives at several very interesting conclusions and sums up his findings on the main points of his thesis something like this: "Many of the investigations hitherto made on individuality and differentiation among primitive people seem to have taken their start from erroneous opposites, such as *individual and group,* and *individualism and collectivism.* The assertion that the individual is more bound by the group in primitive tribes than in our cultured society is incorrect. It may not be maintained that the primitive group has anywhere near complete homogeneity."

The book is a real jewel of clearness within the contemporary social ethnographic literature which so often obscures the subtle qualitative differences by logical deductions from an *a priori* working hypothesis. It is also very refreshing to find an author in this field who evidently is fully acquainted with all modern schools of

thought in psychology. There is no doubt that social psychologists and workers in the ethnographic field might benefit greatly both from the theoretical discussions and from the general method used in this treatise.

Mooseheart Laboratory for Child Research MARTIN L. REYMERT

The Behavior of Animals: An Introduction to Its Study. By E. S. RUSSELL. London, Arnold, and New York, Longmans, Green & Co., 1934. Pp. viii, 184.

This series of lectures delivered at University College, London, before a group of zoölogists and comparative anatomists approaches the problems of animal psychology from a naturalistic as well as an analytical angle. While analytical laboratory investigations are not disparaged, the author believes that their results can be adequately interpreted only in the light of results from systematic field observation. The ecological type of study should precede the laboratory investigation, otherwise "we may easily go astray in our interpretation of behavior in the unnatural conditions of the laboratory experiment; we may easily devise experiments which are meaningless, and, from the animal's point of view, stupid."

It is asserted that each animal actively seeks its "ecological norm" or "ecological niche." The laboratory experiment, in so far as it removes the animal from its "ecological norm," investigates abnormal rather than normal activity. This activity, according to the author, consists in striving to return to or approximate the "ecological norm." Some tropistic behavior evident in the laboratory is thought to be explicable in these terms.

In his criticisms of the analytical laboratory investigation the author fails to distinguish between a comparative psychology, in which we are interested in the animal as such or in relation to other animals, and an animal psychology, in which we are interested in the animal as a living mechanism the stimulus-response functions of which may be subjected to physiological analysis. Ecological studies, it seems to the reviewer, are much more important for the former than for the latter sphere of investigation.

The ecological point of view dominates the lectures, although the results of laboratory investigation are by no means slighted. A good knowledge of the recent work in both fields is apparent. The lectures deal with the following general topics: Methods of approach; behavior and ecology; maintenance activities; characteristics of instinctive behavior; instinctive behavior in relation to development and reproduction; development of behavior and learning; insight learning, and perception; and *Gestalt* theory. Many problems are approached from a configurational and a purposive point of view. In some instances it appears that the author may be acquainted with Tolman's "sign-gestalt" utterances, although no acknowledgments are made in this connection. Sign stimuli, needs, and expectations play a significant rôle. Furthermore, "it is probably safe to say that an animal responds primarily to a perceptual situation, or complex, *as a whole,* and especially in its relation to the dominant need of the moment; . . . it often responds, proleptically, to the probable or usual development of a situation."

The student, the teacher, and the investigator of animal problems will find this little volume both refreshing and stimulating. It is written in an interesting style, is well illustrated, is carefully documented, and possesses a good index.

University of Pittsburgh NORMAN L. MUNN

Psychology in Education with Emphasis on Creative Thinking. By JOHN M. FLETCHER. New York, Doubleday, Doran & Co., Inc., 1934. Pp. xx, 524.

This is an unusually competent discussion of the psychological principles and data upon which educational practice, if intelligent, must be based. Part I deals with acquisition, impression, intake and learning. It contains a thorough discussion of theories and researches bearing on these topics as set forth by various schools of thought, and considers their bearing on education. The author concedes great educational value to behavioristic psychology, but holds that consciousness and purpose are of real significance and that instincts, emergent theories and *Gestalt* psychology must be taken into account under some name if animal and human behavior is to be explained and education intelligently directed. Part I closes with discussions of the curriculum, the importance of emotions in life, and the need for emotional education and mental hygiene in the school.

Part II deals with production, expression, output, creation, claiming that these most important phases of education have been generally ignored or mishandled. Creative thinking is regarded as the most important form of mental activity. The author considers it as quite distinct from reflective thinking which has received considerable attention from psychologists and educators. In this portion of the book facts and the results of research are not ignored, but the treatment is much more theoretical and at times largely speculative and metaphysical. It contains significant quotations from poets and empty generalizations from the supposedly all-wise philosopher, Whitehead. Very strangely there is no adequate discussion of project and activities programs in education which include most of what is being done in the schools to develop creative thinking. In the closing chapter on education as a social process he very properly challenges Allport who says that there is no locus of a group mind to give the locus of the individual mind. The author holds that man is both individualistic and socialistic by nature and must be educated for social relationships, as well as by them, must be prepared for living in a highly coöperative society as well as for individual living.

On the whole, the book is an informal, discriminating and stimulating discussion of fundamental truths of education. It is not prepared milk for beginners in the study of education but is meat for leaders, both actual and in training, who may be expected not merely to administer our educational institutions but to reconstruct and improve them.

References are given in every chapter, and stimulating questions for discussion; and at the close of the book, a glossary and index.

The reviewer finds little to criticize in the carrying out of the plan of the book, but should like to register his protest against the endorsement of the commonly held theory that, because many habitual actions which are at first consciously performed become mechanical, no habits are formed without conscious control in their early stages. Such a view almost compels one to accept the ancestral bee as having been a highly intelligent engineer, who having perfected the process of comb-building, transmitted a modified neuro-muscular apparatus to her descendants in whom it works automatically. The fact that neither the female queen nor the male drone make use of such an apparatus, does not greatly add to the absurdity of the theory.

The reviewer is also a little disappointed that the author spent so much time

in discussing drug-produced and abnormal manifestations of creative thinking, and gave so little space to showing how knowledge of race practices and achievements may be used to stimulate creative thinking, and also to keep it from being bizarre and without value to others.

Leominster, Mass. E. A. KIRKPATRICK

Dynamic Social Research. By JOHN J. HADER and EDUARD C. LINDEMAN. New York, Harcourt, Brace & Co., 1933. Pp. x, 231.

The book outlines an approach to the study of functional groups, and discusses the methodological problems connected with it. The authors exemplify their theoretical analysis with material which they gathered in a study of Joint Committees in Industry under the auspices of *The Inquiry.*

The authors contend that a social situation as represented by a functional group can be adequately studied by ascertaining the motives of the participants in relation to the values which they hold, the circumstances under which the motives operate, the interaction of the participants, and finally the changes in motives and circumstances which result from this interaction. The authors name impulsion, circumjacence, interaction, and emergence as four aspects of a social situation. They regard these as postulates to be utilized symbolically for "the dynamics of social process."

The authors stress the fact that in social data subjective and objective elements are inextricably mixed together. The subjective element cannot be eliminated without denial of purpose, which is an all-pervasive influence in every social situation; without arbitrary selections of facts and the employment of techniques "so naïve in conception as to make final results both untrustworthy and unusable." A distinct contribution is made by the authors in their emphasis upon and elaboration of this point. A 'pure objectivity' in *social* research they combat as a delusion. They propose that the research agent instead of attempting to free himself from social and personal purpose, instead of aiming to be disinterested and exclude his sense of values, cultivate self-awareness as to his purposes and interests, and candidly include his values as well as values found in the situation. That scientific method only which considers the subjective elements, the values and purposes of the observer and of the individuals whom he observes, can reveal significant facts concerning social phenomena.

The concluding part of the book discusses in detail the techniques which are at the disposal of the psycho-social research agent. It contains valuable information on interviewing, participant observing, and case analysis. This earnest and thoughtful book is valuable to students of human behavior for its clarifying contribution to controversial methodological problems.

Columbia University THEODORE ABEL

Differential Diagnosis of Ability in School Children. By DAVID SEGEL. Baltimore, Warwick & York, 1934. Pp. vii, 86.

This book points out the need of reliable individual diagnoses and describes the methods available for evaluating the tests for this purpose. Moreover, the techniques for making reliable differential diagnoses of individuals are collated.

The efficiency of tests and measurements for individual diagnoses may be determined by well attested techniques. Among these Kelley's seems the soundest. In

this procedure the standard deviation of a difference for a given pupil is divided by the usual standard deviation of differences. In general, the smaller the ratio is between these measures the greater the differences between the two traits, and the larger the ratio the smaller are the differences. An illustration is given of the application of Kelley's formula to Gates Primary Reading Tests.

Once the capacity of tests for differential diagnoses has been determined there remains the problem of establishing the meaning of differences scored on sub-tests by each individual. The first method develops the fact that the probable error of the difference between two scores which an individual makes on two tests, is three-fourths of the sum of the tests' probable errors taken separately. This is true only if the differences are chosen at random. Tables are provided giving the chances that any obtained difference will be significant. It is shown that equivalent scores may be got from two tests, if they have been standardized on the same group of children, (1) by drawing a graph using the grade standards, (2) by using the formula $X_1 = M_1 + (\sigma_1/\sigma_2)\ (X_2 - M_2)$, and (3) by using the method of standard scores.

The last chapter contains a list of tests and measurements which may be of value in making a differential diagnosis.

All told, this book contains a careful compilation of methods useful for making accurate differential diagnoses. One wonders why the method of factor analysis developed by Thurstone was not even mentioned when such great emphasis was placed on the work of Spearman and of Kelley.

University of North Carolina A. M. JORDAN

Training the Adolescent. By RAPHAEL C. MCCARTHY, New York, Milwaukee, Chicago, Bruce Publishing Co., 1934. Pp. xx, 298.

This book is written as a text for Catholic colleges. Catholic philosophy is, therefore "appealed to often in an effort to interpret some of the findings of modern psychology" and no apology is offered for "frequent mention of the soul, the free will, and other realities that are either held in slight repute among the majority of present-day psychologists, or are conceived in a radically different sense" from that in which they are employed in this book.

The author is thus forearmed against question of his systematic assumptions, though question is invited on almost every page of the first half of the book. One sample: "One of these distinctive human abilities is intelligence. It is the manifestation of the working of the intellect. There is a difference, therefore, between intelligence and intellect. The latter is a faculty or power, the former is a function. . . . The idiot has an intellect but it does not function, so he has no intelligence." This in a college text in the twentieth century! There are no references to source material. The occasional foot-note references are almost without exception to general treatises. Statistics are omitted because these are "readily available" elsewhere. There are suggested readings and topics for discussion for each of the nineteen chapters. The reading references are almost exclusively to other text-books or popular homilies. The questions provide abundant opportunity for the expression of opinion but slight incentive to the search for facts or to critical evaluation.

While there is much of common sense and of fine idealism in the book, the discussions are characteristically general and dogmatic in character with little of illustra-

tion or concrete practical application. It may be assumed that the text will admirably serve its announced purpose. That it will be useful outside this limited sphere may be questioned. It assuredly will not introduce the college student to the scientific consideration of this phase of genetic psychology. Nor will it furnish to parents, teachers, or other workers with adolescents anything beyond well ordered generalizations and injunctions.

Emory University — G. C. WHITE

Die Ganzheitspsychologie Felix Kruegers. Methodische Grundgedanken und grundlegende Ergebnisse. By ONKO BUSS. München, C. H. Beck, 1934. Pp. 60.

This little brochure, as one gathers from its title, serves as an introduction to, and summary of, the fundamental concepts of Felix Krueger's type of psychology which is broadly to be reckoned with the totality psychologies now familiar to everyone. Perhaps the chief points of difference with *Gestaltpsychologie* lies in the emphasis on the feelings and the broader connotation of *Ganzheit* which Buss (and Krueger) regards as including the narrower term *Gestalt,* the meaning of which is restricted to 'formedness.' The exposition of Krueger's principles makes clear how this particular brand of totality psychology helps to understand social phenomena and lays the psychological foundations for the justification of the present political régime in Germany. Anyone interested in the concrete, experimental work done by the Leipzig school will get no hint of it in Buss's account of Krueger's psychology.

Bryn Mawr College — HARRY HELSON

Methoden der Psychologie des Gefühlslebens. Handbuch der biologischen Arbeitsmethoden. Abt. VI, Methoden der experimentellen Psychologie, Teil B, 2. Hälfte, Heft, 2, 3, u. 4. By GUSTAV STÖRRING. Berlin and Wien, 1931. Pp. 1125-1646.

In spite of the sub-title, indicating that this work forms part of a handbook including the methods of experimental psychology, the treatment of the affects and emotions is largely anecdotal and from the psychoanalytic point of view, only incidentally from the experimental approach. While the experimental attack on the feelings has not been as fruitful as it has been in other fields of psychology, and the author's treatment therefore partially justified, it is a bit surprising for the reviewer to come upon a chapter entitled "The significance of feeling states for the consciousness of the freedom of the will" (p. 1431). The reader interested in the so-called 'dynamic' view of behavior may, however, find these volumes profitable. At least they may serve to suggest problems capable of experimental attack.

Bryn Mawr College — HARRY HELSON